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1902

A paper read at the 181st Meeting of the American Institute of Electrical Engineers, New York, February 14th, 1902. President Steinmetz in the Chair.

STATIC STRAINS IN HIGH TENSION CIRCUITS AND THE PROTECTION OF APPARATUS.

BY PERCY H. THOMAS.

It is the purpose of the present paper to discuss the so-called "static effects" in high tension circuits, especial attention being given to the disturbances produced by lightning, switching, grounding, and the like. Some discussions of particular phases of the principles involved have been published from time to time, but so far as has come to the attention of the author, no comprehensive treatment of the subject of a non-mathematical character has so far appeared. On this account a general view of the question without mathematical complication will probably prove of interest to superintendents and station managers as well as to electrical engineers. Necessarily much of the matter in the paper will not be new, but it will be found that the old principles when applied to commercial circuits yield quite novel and important results.

It is scarcely necessary to state that static strains, though not perhaps particularly destructive of themselves, may indirectly cause the loss or temporary disabling of expensive and important apparatus; and more is involved than the loss of the apparatus. One of the principal aims, if not the chief aim of the superintendent of a modern electric transmission system, for the accomplishment of which he will make great sacrifices, is to keep his service continuous. Static disturbances in such systems, when not properly controlled, are a constant menace to the continuity of the service. The prominence of static effects is one of the chief distinguishing characteristics of high tension operation. The importance of "static" is not emphasized solely on theoretical grounds. Plants in actual operation have suffered both damage to apparatus and loss of prestige on account of insufficient static protection.

The paper will be divided into two parts:

I. Nature, Causes, and Effects of Static Disturbances; and

II. The Protection of Apparatus and the "Static Interrupter."

PART I.—NATURE, CAUSES, AND EFFECTS OF STATIC DISTURBANCES.

Introductory.

The term "static" is certainly a misnomer, as the most prominent characteristic of the phenomena thus designated is the abrupt and violent nature of their discharges of electricity. However, this group of phenomena is so clearly defined and is so generally recognized by this name that in the absence of a better term of general acceptance its use will be continued. A new term, however, is very greatly to be desired, and as the phenomena described are essentially those of wave motion, the author recommends the word "*undic*" as a substitute for "static."

For the present discussion "static" (or "undic") will be taken to include those changes of potential and waves of current of an abrupt nature which result from the transfer of the electrostatic charges of a system from point to point. Such actions are not directly produced nor controlled by the generator. I say "not directly," for, of course, practically all electrical phenomena in a circuit, except those due to lightning, are produced at least indirectly by the generator.

Every electric circuit may be viewed in two ways. It may be regarded (as in dealing with the transmission of power) as a closed circuit containing resistance and inductance, and carrying a certain current produced by a certain e. m. f., or it may be regarded (as in dealing with static) as a large insulated "conductor" (as the term is used in treatises on static electricity) of peculiar form. That is, the circuit performs a double function. It transfers useful energy and at the same time acts as an electric condenser. The first aspect, being the one of greatest practical importance, is the one almost universally considered. However, even in this case, with long high tension lines, it has been found necessary to give some attention to the e. s. (electrostatic) capacity of the circuit taken as an insulated "conductor." The second aspect of the circuit is the one to be here considered, and though of less importance than the first it still deserves careful consideration on account of the serious difficulties static may cause

in commercial work. As a matter of fact, since the electric circuit when operating is at the same time both a closed circuit carrying current and a charged insulated "conductor," the actual resultant condition of the system is a combination of these two states.

There are some points of difference, however, between the commercial electric circuit and the insulated "conductors" commonly used in the laboratory demonstrations of static electricity. In the first place the commercial circuit is very much the larger and is extended over greater distances, so that a sensible time is required for an electric impulse to pass from one end to the other. Consequently two points on the same conductor may be momentarily at very different potentials. In the second place the amount of energy stored in the e. s. capacity of the commercial circuit is very much larger than that in the "conductors" of laboratory experiments. It should be noted that these points of difference are both difference of degree, not of kind.

It will be well at the outset to summarize briefly the most characteristic of the laws of static. These are all well known and of very general applicability, viz.:

Laws of Static.

I. A static discharge (which is merely a very sudden rush of electricity) encountering a choke coil in its path, experiences momentarily great opposition, *i. e.*, the electricity is temporarily held back by the inductance of the coil, so that if the rush of electricity be of large volume and sufficiently sudden, a very great e. m. f. (neglecting losses of energy) will be exerted on the choke coil. This high e. m. f. is but momentary, however, as the choke coil after the first instant allows the electricity to pass at an accelerating rate which soon relieves the pressure. The high momentary pressure at the front of the choke coil causes a tendency for the charge to seek other paths. Such a phenomenon would be called "side flash" and is one of the considerations which led to the use of choke coils with lightning arresters. Lightning arrester choke coils are so placed that any static disturbance on the line must pass the choke coil to reach the apparatus which is being protected, and the arresters are placed on the line side of the choke coils where the tendency to side flash is strongest.

As there is a tendency to side flash toward other objects, there must evidently also be a tendency to short-circuit the choke coil

itself. Such short-circuits are by no means unusual when coils not designed to withstand them, are exposed to static discharges.

II. All conducting bodies have e. s. capacity or in other words are to a greater or less degree condensers. Whenever a conductor is at a potential different from a neighboring conductor, a charge of electricity appears in its surface principally on the side toward the other body. The amount of this charge equals the product of the e. s. capacity and the potential between the bodies. At the same time an exactly equal charge appears on the second body. A conductor may have capacity to two or more adjacent conductors at the same time; in this case its resultant charge is the sum of the separate charges.

The fact that a certain quantity of electricity, great or small, is required to charge any conductor to any definite potential is very important in its bearing on long distance lines. Suppose for instance that a piece of apparatus at one end of a transmission line is to be raised to a certain potential by a sudden application of voltage at the other end. A sensible time will elapse before the necessary charge for the apparatus can reach the end of the long line. Consequently the potential of the apparatus will not rise at the same instant as the applied e. m. f. but will remain unchanged until charge actually reaches it. In other words, the potential of a condenser cannot rise until the charge necessary to produce this rise has entered it.

The large current required to charge the line wires of an extensive high tension system is well known. All switches, transformers, instruments and other apparatus actually connected to the high tension circuit also require charge, though of course much less.

The line wires have e. s. capacity both to the earth and to one another. The several charges corresponding to those different capacities though superposed on the wire, act independently of one another and must be so studied. Similarly with apparatus connected to the lines, not only will there be e. s. capacity in the high tension parts to ground and low tension parts, but also between different parts of the high tension windings themselves.

III. When a current is flowing through a circuit containing inductance, energy is stored in its magnetic field. The amount of this energy equals one-half the product of the inductance and the square of the current. This energy was obtained from the circuit when the current was started, and when the e. m. f. producing the current is removed, must be discharged back into the

circuit before the current can stop. If the circuit be suddenly opened, a very high potential will be developed to keep the current flowing until this energy is fully discharged. Such is the case of the well known rise of potential which results from suddenly opening the field circuit of a large generator. The direction of this "extra" e. m. f. is such as to continue the current flow.

Energy in a magnetic field is just as truly stored energy as that in the moving bullet, the stretched spring, or the raised weight. It may be adapted to useful purposes. For example, the energy stored in the magnetism in the core of an induction coil by the primary current, is discharged into the secondary to produce the high tension spark.

An idea of the amount of this energy in actual cases may be obtained from the following:—one ampere flowing through a coil w with an inductance of 1 henry (equivalent to 377 ohms at 60 cycles) stores 1/2 volt-ampere-sec = 1/2 watt-second, = 1/2 joule = 0.367 foot-pounds.

The very sudden rushes of static electricity are no exception to this law; they produce magnetism and store energy which must be discharged into the circuit again before the currents can cease. This principle will be found to lead to important results.

Also, when a condenser is charged, energy is stored. This energy will be restored to the circuit again when the condenser is discharged. The amount of this energy is one-half the product of its e. s. capacity and the square of the voltage.

A condenser of one microfarad capacity charged to 1000 volts potential stores 1/2 a watt-sec = 1/2 joule, = 0.367 foot-pounds.[1]

1. The transmission lines of a system like the Bay Counties Power Co. of Cal., with lines 150 miles long, have a capacity of about 3 microfarads. Operating at 40,000 volts this will have $1/2 \times (3 \times 10^{-6}) (40{,}000 \times \sqrt{2})^2 = 4{,}800$ watt-secs = 4,800 joules, = 3,500 foot-pounds of energy stored in its e. s. capacity when fully charged. The charging current is 45 amperes at 40,000 volts, 60 cycles. Therefore the rate of supply of energy to the line from the generator and absorption from the line by the generator is $45 \times 40{,}000 \times 2 \times \frac{1}{\pi} = 1{,}150{,}000$ watts, = 1,150,000 joules per sec., = 843,000 foot-pounds per sec. The generator supplies current continuously to the line for 1/4 cycle and then receives it back again for the next 1/4 cycle. Therefore the energy delivered or received in a half alternation is the energy stored in the capacity of the line, $= \frac{45 \times 40{,}000 \times 2}{\pi \times 60 \times 4} = 4{,}800$ watt-secs. = 4,800 joules = 3,500 foot-pounds, as before.

The energy of an electric current may thus be stored in two ways: either in a magnetic field produced by a current, or by the storing of the electricity itself. In its former state the energy may be regarded as kinetic and in the latter as potential.

Electric Circuit as an Insulated Conductor.

It has been stated that an electric circuit may be considered as an insulated conductor of a peculiar form. It is rather an aggregation of conductors of various forms. In general a high tension circuit consists of the following parts:

(*a*) High tension windings of transformers, raising and lowering.

(*b*) Transmission wires.

(*c*) High tension switches, instruments, lightning arresters, station wiring, and similar auxiliary apparatus.

(*a*) The high tension transformer windings are in the form of coils, and are therefore from the static point of view choke coils. On account of the very large number of turns their choking power is very great (choking power in general, being proportional to the square of the numbers of turns). The high tension windings have e. s. capacity also, as they lie close to the low tension windings and to the core, that is, the high tension winding will have static charges on the surface opposite the low tension winding and opposite the core. There is as well, capacity between turns and between coils of the high tension windings. The e. s. capacity of these transformer coils is of a rather complicated nature, for it is distributed along at different points on the winding, and is not concentrated at one point as in an ordinary condenser. Therefore the charge for the different portions of the windings must pass through different lengths of wire and different amounts of inductance and resistance. The same is true of both raising and lowering transformers.

(*b*.) The transmission wires have e. s. capacity with one another and with the earth, so there are two or more charges superposed on each wire—each of which may act independently of the others. This capacity, though comparatively small for short lengths of wire is, as a whole, large, on account of the great length of the lines. These wires have inductance also, like the capacity; small for short lengths, but quite considerable when the line is taken as a whole.

On the line wires, as in the high tension transformer windings, the e. s. capacity and inductance are distributed, so that when voltage is applied to the line, different points receive charging current at different instants of time, since the charge for various points must flow through different lengths of line. In fact, the transmission wires may be considered as a succession of choke coils and condensers in series, each very small, but very great in number, as shown in Fig. 1.

In addition to the e. s. capacity of the line wires, there is a certain amount of capacity on each insulator. The amount is very small and may be considered a part of the line capacity.

(*c.*) Switches, instruments, lightning arresters, station wiring, and similar auxiliary apparatus have a certain amount of e. s.

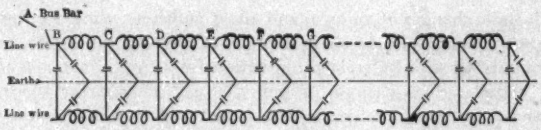

FIG. 1.—Diagram illustrating Inductance and Capacity of a Long Single Phase Transmission Line.

capacity to surrounding objects, but not usually as much as the high tension windings of transformers. This part of the system has also comparatively little inductance and needs no further discussion here.

In addition to e. s. capacity and inductance, all parts of the high tension system have more or less ohmic resistance. The effect of this resistance is to retard the current (static or normal) in whatever direction it flows, and to change part of the electric energy into heat.

A certain amount of current leaks from the line at insulators and at all points where insulating material touches the circuit, since no insulation is perfect. Even the air conducts away some charge when the voltage is extremely high. Taken altogether the result of these leakages is small under normal conditions, and may be neglected in this discussion.

Every point of a high tension circuit at any instant has a perfectly definite potential, which is in general different from the

potential of other parts of the circuit and of adjacent objects. Consequently each point has at each instant a definite static charge, often different from that of other points. As (with alternating currents) the potential of each point is varying from instant to instant, these charges are continually changing and the changing charges cause a current in the circuit properly called charging current. Neither this charging current nor the static charges have any connection with the useful current except as the latter may influence the static potential of some point of the circuit. The total current in the circuit at any instant is the sum of the work current and charging current if they are in the same direction, or their difference if they are in opposite directions.

When the potentials of all points of a circuit are determined directly by the electromotive force of the generator, *i. e.*, during normal operation, all changes of potential are slow (compared with static changes of voltage) and allow sufficient time for the necessary changes in the static charges at various points to be accomplished without serious opposition from the inductance of the circuit. Such changes of potential and charges as these, however, are not the subject of this paper. Its principal object is the discussion of changes of potential of a more abrupt nature which are not directly produced by the e. m. f. of the generator; the latter may cause violent and sudden alterations of the static charges and dangerous local potential strains. However, before taking up the more complicated case of the abrupt changes, it will be best to consider a little more fully, the distribution of potential and charge in some of the simpler cases of normal running, viz.:

Static Charges on a Symmetrical Single-phase Transmission Line, Open-Circuited at the End and Charged by a Direct Current Generator.—In this case both lines have the same e. s. capacity to the earth. There is also capacity between the line wires. On the positive line is a positive charge composed of two parts, one due to its capacity to earth and the other to its capacity to the other wire; similarly on the negative wire is an equal negative charge composed of two parts. These charges remain constant as long as the e. m. f. of the generator is constant.

If, however, the generator be an alternator, the charges change from positive to negative with the voltage, remaining equal on the two wires at all times. This interchange of charges requires a flow of current from line to line through the generator.

When the same line is loaded at the farther end no material change is produced in the line charges. Since different parts of the high tension winding of the motor or transformer or other apparatus constituting the load, are at potentials varying all the way from one-half line voltage positive to one-half line voltage negative, the charges on these points will vary in a similar manner. These charges are supplied from the generator through the line. The charges for the inner parts of the winding of the load apparatus must flow through the outer parts of the winding as well as through the line.

Same Line with One Leg Grounded.—Since direct current is practically never used in high tension work it will be omitted from further discussion here. The case of a grounded line is of importance not as a practical operating condition, but on account of the harmful results which may follow accidental grounds. The potential of one line wire becomes the same as that of the earth, while that of the other is full line voltage double its value with circuit ungrounded. The potential between the wires being maintained by the generator remains unchanged. Of the two component parts constituting the charge on the ungrounded wire, the part due to the e. s. capacity between the wires is undisturbed by the grounding, since the voltage between wires is unchanged, but the part due to the capacity to earth is doubled since the potential of the line above the earth is doubled. On the grounded line the part of the charge due to the e. s. capacity between wires is unchanged—the part of charge due to capacity to earth becomes zero, since the potential to earth is zero. The charge which appears on the earth opposite the ungrounded wire and which is equal in amount to the part of that line's charge that is due to capacity with the earth, being produced by the generator must flow to earth at the point where the circuit is grounded. On a large system the e. s. capacity of the lines would be sufficient to make this current quite large. In such a case the current may burn off the line wire at the grounded point if the contact with the earth is imperfect. This fact has a very direct bearing on the question as to whether it is possible to operate a large high tension system with one wire accidentally grounded.

System with Neutral Point Grounded.—In a symmetrically arranged ungrounded system either single phase or polyphase, a neutral point may be defined as any point which is at the earth's

potential. There may be more than one neutral point in a circuit as shown in Fig. 2, where *a b c d* are neutral points.

Since the neutral point is at the earth's potential, grounding this point makes no difference in the potential or charge of any point of the circuit, and no current flows over the grounding wire. If the circuit becomes unbalanced, for example by a high resistance ground on one wire—there is a tendency for the neutral point to take a potential different from that of the earth, and a current will then flow over the grounding wire which will be sufficient to keep the neutral point at earth's potential. If there be a dead ground on one wire the earth connection at the neutral

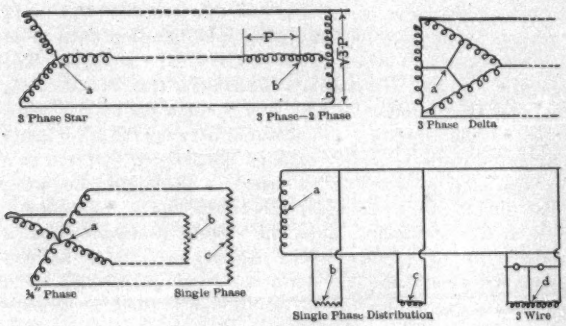

FIG. 2.—Neutral Points.

point completes a short-circuit. If the neutral point is not grounded when the system becomes unbalanced, one or more lines will take a potential higher above the earth than in the balanced condition and the other line or lines a potential lower than before. Therefore, comparing the two methods of operation;—with the neutral grounded, if a "dead" ground is made on any line wire, a short-circuit is produced through the ground connection at the neutral point; with the neutral undergrounded a ground on a line wire means approximately twice the previous potential between line and ground on one or more wires though no short-circuit is produced. The advantage of a grounded neutral is the preventing of increased potential over the earth in case of an accidental

ground, its disadvantage is the fact that one such accidental ground instead of two, causes a short-circuit.

We may now consider some of the abrupt changes of potential not directly produced by the generator.

1. *Connecting "Dead" Transformer to Live Line.*—Fig. 3 represents the portion of a high tension transformer coil connected to the line terminal. The circles indicate wires. The coil may have one or more turns per layer. The numbers on the circles indicate the order in which the current passes through the turns of the winding. The low tension winding lies adjacent to the high tension and the small condensers *a b c d*, etc., shown dotted, indicate the e. s. capacity of the corresponding wires to the adjacent low tension coils.

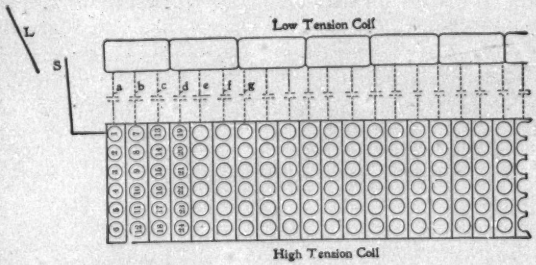

FIG. 3.—Diagram Illustrating e. s. Capacity of a High Tension Transformer Coil also Charging from a Live Line.

Before either terminal of the transformer is connected to the line, all parts of the high tension winding are at earth's potential. As the first line switch is being closed at s there is no change in the potential of the transformer winding until a spark passes between the switch jaws. Then instantly the potential of the transformer terminal is raised to the potential of this line wire. The rest of the high tension transformer winding also takes the potential of this line wire as soon as (but not sooner than) the necessary charge can reach $b - c - d - e$, etc. Now, the charges for $b - c - d - e -$ etc., must flow through a considerable amount of inductance, which will require a length of time which is very short but still sensible. It is clear that during this short but definite period, after the terminal has reached its full potential and before there has been sufficient

time for charge to reach the inner layers, for example, f and g, etc., a difference of potential exists between the outer and inner layers of the winding which is equal to the full e. m. f. of the line wire above the earth. If the insulation of the winding is too weak, or the line potential is sufficiently high, this momentary difference of potential will cause a spark to pass over the surface of the coil or through its insulation. This spark contains a very small amount of electricity, for as soon as the wires, f, g, etc., are charged up to line potential, the voltage between the inner and outer layers vanishes. The only result is the almost harmless passage of a nearly invisible spark.

Very soon after the closing of the first line switch the whole transformer winding will have assumed the potential of the first line wire, that is, just before the closing of the second line switch the second transformer terminal is at the potential of the first line. As the second switch is closed, a spark passes, and the potential of the second terminal of the transformer which has up to the instant been at the potential of the first line is suddenly changed to that of the second line—a very abrupt change. Then, as before, during the period required for the necessary charge to penetrate to the inner turns, a very high potential difference is impressed on the outer portion of the coil. The momentary strain on the insulation of the coil is greater when the second switch is closed than the first, for the first transformer terminal experienced an abrupt change only from earth's potential to line potential while the second was changed from the potential of one line to that of the other line, which is nearly double the potential from the ground.

There is another very important difference between the effects of closing the first and second switches in connecting a "dead" transformer to a live line. In the case of closing the first, as already stated, if the momentary strains break down the insulation, only sufficient current flows through this break to charge up the inner layers of the transformer coil. This is a very small quantity and can do comparatively little injury to the coil, especially if it be oil-insulated. In the second case the amount of current passing in the static spark is not very materially greater than in the first case. But when the insulation between turns is momentarily broken down by this small spark, there flows through the break a certain amount of current due

to the e. m. f. impressed by the generator on each turn of the coil. Although the static spark of itself would be but momentary, yet the current supported by this impressed or "normal" e. m. f. of the circuit may be able to hold the arc and continue indefinitely, destroying the whole coil if not interrupted. I say *may* be able to hold an arc, for such an arc may or may not be held, according to the circumstances of the case. If the static spark passes when the normal e. m. f. is nearly zero, or if the transformer be not able to supply much current in short-circuited turns, etc., then the chance that a permanent arc will be established is small. Sparks or flashes have actually been observed in the winding of high tension transformers at the time of lightning discharges, showing actual temporary holding of an arc.

The factors which determine the minimum number of layers upon which excessive momentary potential will be impressed are, chiefly, the inductance and e. s. capacity of the transformer coils and the abruptness of the change of potential of the terminal on the closing of the switch. The former determines the rate at which the charge can penetrate the coil, and the latter determines the time during which charging current may be passing into the coil before the full potential is reached on the terminal. The more abrupt the spark and the greater the capacity and inductance of the coils, the fewer the number of layers which will become charged before full terminal potential is reached, and the more severe will be the strain on insulation.

When switching is done on a high tension generator or a motor or any apparatus containing coils, a strain is brought in the windings near the terminals in exactly the same way as with transformer coils. There is also, usually, this same tendency for normal voltage to cause an arc to follow any momentary break in the insulation.

It is evident that the danger from this sort of switching is greater and greater for higher and higher voltages—it is of little importance on low voltages. Actually, injury of a serious nature to apparatus from this source is very much less than would be at first expected for several reasons:

(*a*.) Insulating materials will stand much higher voltages of a static nature than of a continuous nature, such as those derived from generators.

(*b*.) The passing of a static spark alone is usually by no means a serious matter and many circumstances may prevent a destructive arc from following the spark.

(c.) It is only occasionally that the combination of circumstances arises which gives the severest conditions.

It has been assumed thus far that the potential of the line wires is unaffected by the switching operations. If, however, the connecting of the transformer to the line momentarily lowers the line potential at the switch, as will often occur, the abruptness of the static strain, and therefore its severity will be reduced.

This discussion which has been applied to switching on a transformer, is applicable when any coil is subjected to an abrupt change of potential in any part. For example, when a dead transmission line is connected to a live transformer the potential of the transformer terminals will usually be suddenly lowered very considerably for the moment, on account of the great excess of e. s. capacity in the line over the transformer coil. In this case the potential of the line which must be charged directly by the generator (and not from charge previously stored in the e. s. capacity of other lines) will rise very much more slowly, since current must pass through considerable inductance in generator and transformers. Sudden short-circuits, grounds, discharges of lightning arresters and similar disturbances all produce static strains more or less severe according to circumstances.

Fourth Law of Static.

Before continuing the consideration of other cases of serious static it will be best to consider one or two more fundamental cases:

IV. When a condenser discharges suddenly through an inductive circuit containing comparatively little resistance, the current in the discharge path increases steadily until the condenser is completely discharged. At this time the current, in virtue of the magnetic field which it produces, stores nearly the full amount of the energy originally in the condenser, for there is little waste, as the resistance is assumed to be small.

But this current cannot become zero until all its energy is discharged into the circuit, which in this case means into the condenser; therefore the condenser is charged to its original voltage again, but this time in the opposite direction. At the instant all the energy is thus restored to the condenser (with reversed direction) the current is zero. But the condenser cannot remain

charged with the discharge path closed, and will discharge again, repeating the same phenomena; that is, the charge oscillates backward and forward through the discharge circuit. If the resistance is not zero, a little energy is lost every time discharge occurs, so that each time the condenser is charged to a lower potential than the preceding time until the whole of the energy is finally transformed into heat. If now a condenser be charged through inductance, energy will be steadily stored in the magnetic field produced by the charging current (just as in the case of discharge) until the condenser has reached full potential. All this energy must be discharged into the condenser before the charging current can cease; that is, if there are no sensible resistance losses, the condenser will be charged momentarily to double potential. The condenser will then oscillate between double potential and zero until it finally settles down at the potential of the circuit from which it is charged, which occurs only as the oscillations are damped by the resistance of the circuit or by other losses. These phenomena may be made clearer by a mechanical analogy.

The first case of the discharging condenser is similar to that of a pendulum which has been drawn aside and released; it oscillates about its final position of rest until its energy is all expended in friction. The second case may be compared to a weight supported by a spring. If the weight be dropped it will descend beyond the point at which it is finally to come to rest, stretching the spring to twice its final extension. The weight will then be drawn up again above the point of equilibrium, oscillating backward and forward until its energy is dissipated by friction. The inertia of the weight corresponds to the inductance of the charging circuit and the elasticity of the spring corresponds to the capacity of the condenser.

Charging a Short Transmission Line.—Consider the charging of a short transmission line. The line may be considered to be a condenser; assume that it is being charged from bus bars rigidly maintained at constant potential. There will always be a certain amount of inductance in the path through which charging current must pass to reach the line, so that we have the case of the charging of a condenser through inductance with more or less resistance in the circuit. Therefore, when the line is connected, it rises to double potential (neglecting losses) and immediately starts to oscillate between this point and zero until the

oscillation gradually dies out and leaves the line at normal potential.[1] This means that at all points the insulation of the line will receive a strain of double potential. If, on the other hand, the bus bar from which the line is charged has not the capacity to deliver almost instantly the amount of electricity necessary to charge the line, the first effect of closing the switch will be not to raise the potential of the line to its full amount, but to make both line and bus bar take an intermediate potential so that the line comes up to normal potential by steps. Under these circumstances the maximum of the oscillations is materially reduced, and the line is not subjected to double potential. In the extreme case in which the bus bar has a comparatively small capacity to deliver current, the first effect of connecting the line is to bring the potential down to zero. In this case, if the line be charged from transformers, the outer portions of the windings are subjected to severe strains, as already explained. For a bus bar to be able to supply charging current to a "dead" line so quickly as not to have its potential momentarily dropped, it must have the necessary amount of electricity already stored in condensers connected directly to the circuit. In commercial plants the place of such condensers is supplied by other live lines connected to the bus bars.

The strain of double potential produced by charging a line has an interesting analogy. If a piece of metal be tested for tensile strength by suddenly applying a weight on the end, a double strain will be momentarily given the metal due to the slight motion of the weight allowed by the stretching of the test sample. In this case the inertia of the weight corresponds to the inductance through which the line is charged.

The discussion so far assumes that there are no losses of energy in resistance or in currents set up in adjacent bodies. Such losses, which always exist to some extent, tend to reduce both the amplitude and the number of oscillations.

Charging a Long Transmission Line.

In the case of a long transmission line, however, the line cannot properly be considered to be a simple condenser, for this is equivalent to the assumption that the line is so short that its inductance is practically zero, and in actual long lines such is not the

1. This rise of the line to double potential on charging was mentioned in Mr. Steinmetz's paper before the Institute August, 1901.

case. To present such a line, however, we may take a succession of choke coils and condensers connected in series, as in Fig. 1. Consider a single line wire open circuited at the receiving end.

The "dead" line is to be charged at the end, B, from the high tension bus-bar, A. Assume that, as the switch at A is closed, the point B is instantaneously raised to full potential. If now the line extended no further than the condenser C we should have the case of a short line which has just been discussed; that is, the line will rise to double potential and oscillate until it finally settles down at normal potential. But since the line shown in Fig. 1 does not end at C, as soon as the potential of condenser C begins to rise, current begins to flow to condenser D, and as C rises higher and higher more and more current will flow to D. The potential of D then begins to rise, which starts current to E, etc. As D rises slowly at first, C reaches bus-bar potential before D, and similarly D before E, etc. As soon as C reaches this potential it remains constant. At the same instant that C reaches bus-bar potential the current value in the coil between B and C reaches a maximum and becomes constant. Similarly with the currents in the other coils. This final current, which appears in more and more coils as the line charges up, supplies the charge that is being added constantly at those points where the potential is changing, to continue the process of charging, though the potential of C remains constant, that of D continues to rise until it reaches the same value as C, when it too becomes constant, though the potential of E continues to rise, and so on along the line. Similarly with E and F, but each successive condenser reaches its maximum a little behind those nearer the point A, so that the net result is a wave of e. m. f. starting at the point B and passing along the line.

The general distribution of the potential of the line showing the wave form at short intervals of time after connecting on the line is shown in Fig. 4.

This wave passes along leaving the lines fully charged. If the line be infinitely long it will experience no further disturbance, and if there are no resistance or other losses the wave will pass along to an indefinite distance, keeping its form and raising in turn all parts of the line to the full bus-bar potential. If there be considerable losses of energy as the wave proceeds (as there usually are in any actual circuit) this wave will lose its shape somewhat, and will get feebler and feebler, until, if the line be

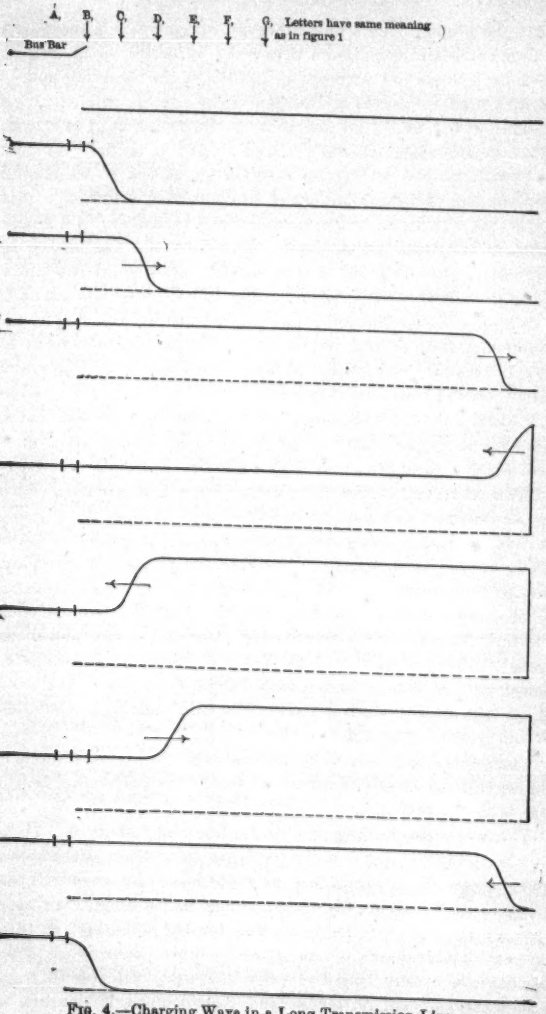

FIG. 4.—Charging Wave in a Long Transmission Line.

long enough, it ceases to be perceptible. This dying away of intensity may be slow enough in commercial lines, so that a large part of the original intensity of the wave will remain when the end of the line is reached. Now the end of the line is open circuited and the wave of electricity can go no further and is reflected back. At the reflecting point the maximum potential reached is twice that of the wave.

On being reflected, the wave immediately starts back along the line, leaving it charged to double potential (assuming no losses), and finally reaches the starting point, A, where its energy is absorbed by the bus-bar. But the line cannot stay at double potential, and a second wave is sent along the line, bringing it back to normal potential again. This second wave will be reflected at the open end, and will return again, but this time dropping the potential of the line to zero until, as the wave reaches A again, the line is practically in its initial condition. This wave, in turn, is absorbed by the bus-bars, and the whole series of operations gone over again and again. If, however, there are resistance and other losses the wave is growing thinner and thinner all the time, and finally dies away, having only partly charged the line. As the wave dies down by the resistance losses an infinite number of small supplementary waves are sent out, which complete the charging of the line. If there are no losses, however, no supplementary waves will be formed.

It will thus be seen that the charging of a long line is much the same as a short line; in both cases the line oscillates between zero and double voltage until the losses cause it to settle down at normal potential. These wave changes are indicated in Fig. 4.

Actual tests on commercial transmission lines suddenly charged have shown a rise of potential much greater than normal at the end farthest from the switching.

Suppose a stretched flexible cord fastened at one end and held in the hand at the other. A quick motion of the hand sideways will send a loop or wave along the cord which will pass to the end, will be reflected back and will finally reach the hand again. This wave is analagous to a static wave in the transmission line

Charging a Branch Line.

Consider a line consisting of two parts—the more remote having a much smaller capacity and larger inductance than the nearer. When a wave starting from the beginning reaches the junction it will be partially reflected, since the whole charge of

the large line cannot be crowded into the capacity of the small line, especially in view of the increased inductance of the new portion; that is, there will be a rise of potential at this point and a wave smaller than the outgoing wave will start back. As the original wave will not be wholly reflected, the potential at the juncture of the two circuits will not be double the charging voltage.

But a wave will also be sent forward into the second part of the line which will have a crest as high as the maximum of the potential at the reflecting point. Therefore, at the end of the second line, where another reflection occurs, the crest of the last wave will be doubled. The resultant potential at this point will thus be very high, but not over four times the original voltage of the bus bar. It may be much less. This means that a branch line at the end of a main system (especially if the latter consist of two circuits in multiple) will receive a very severe shock at the farther end when a wave enters it from the main line. This is a case that may readily occur in actual plants and should be carefully considered. If there be a third line on the end of the second leaving a still less capacity and still greater inductance a wave will be formed in this line whose crest has the value of the maximum rise of the reflecting point at the end of the second section of line, and which will double its potential at the farther end as before. This last rise of potential has as its maximum theoretical limit eight times the original charging voltage, but would actually always be much less. This case is very unlikely to occur in actual circuits. The total energy in the waves in the second and third sections of lines is much less than in the original wave, but is at a higher potential.

To return to the consideration of the uniform circuit. It has been shown that when one end of a long line is suddenly raised to a certain potential and maintained there, a wave of charge passes along the line, leaving it charged, and that when this wave reaches the open end of the line it is reflected and produces all along the line a potential double that of the bus bar. If, however, a sine e. m. f. be momentarily applied to the line so that the end of the line is not maintained steadily at the full potential as before, but is immediately lowered a wave will be found in the line as before with the same maximum voltage but a different form. (See Fig. 5.) This wave leaves the line uncharged. The rise of potential at the reflecting point, how-

ever, is the same as long as the crest of the wave is the same, but in this case only the reflecting point receives double potential (assuming that only a single wave is sent into the line).

If a wave be started at one end in a very long narrow trough of water, it will proceed the length of the trough and on reaching the end will be reflected and will rise up to double height at the reflecting point. This wave in the water is again analogous to the wave of charge passing along the line, leaving it unchanged and causing a double strain at the reflecting point.

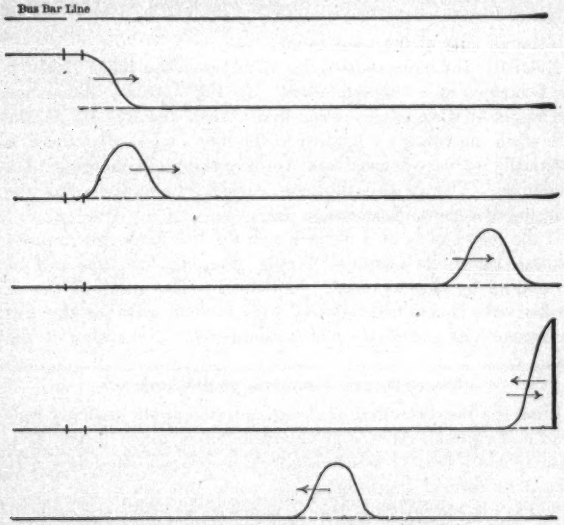

FIG. 5.—Charging Wave in Long Transmission Line.

In all cases of static effects the change in e. m. f. of the bus bars due to the generator is so slow that the generator e. m. f. may usually be considered constant while the static phenomena are taking place.

The length of the wave in the line is determined by two factors—the speed at which the charge passes along and the time required to bring the potential of the first point of the line to its full value; it is the extreme distance to which charge has penetrated when the charging potential becomes stationary.

If the line is shorter than the length of the wave no complete wave will be formed, but reflection will produce the same rise of double potential at the end. The speed at which the wave will pass along the line is inversely proportional to the square root of the product of the capacity and the inductance per unit length, (unit length = 1 earth quadrant = 6,200 miles)—that is, to the time constant. But on air lines the waves always travel at approximately the speed of light, so that there is an inherent relation between the inductance and capacity of an air line which at first thought is very surprising. This relation is expressed in general by the equation $v = 1/\sqrt{L\,C}$, or $L = 1/v^2\,C$ where v is the velocity of the wave in air.

Evidently the more sudden the disturbance the more likely is the formation of a complete wave. In the extreme case when the length of wave is very much longer than the line (as in the case when the voltage is applied to the line very slowly) there is practically no wave formed and we have the plain charging of a condenser. This is also the case considered in discussing the charging of a short transmission line.

If the source of e. m. f. from which the line is charged cannot maintain rigidly its potential during charging, the line will be charged up by steps as already explained. The result is that a weaker wave is obtained followed by a second wave as the bus bar recovers its potential—which completes the charging of the line.

Charging an Underground Cable

What has just been said of charging a line applies equally well to an underground cable, only the cable has a much larger e. s. capacity and a smaller inductance than the transmission line. If a wave be formed in charging the cable this wave will be reflected at the end, causing double potential all along the cable as it returns to the starting point. If, however, the cable be so short that no wave be formed the cable will be charged like a short line, that is, as though it were a condenser. As before, if a momentary impulse up and down be given the cable instead of a steady charging voltage, a wave of the form shown in Fig. 5 will be produced which causes double potential at the end of the cable and there only when but one impulse is sent into the cable.

If a steady alternating e. m. f. is applied to a transmission line, waves will be sent along having crests of positive and nega-

tive values alternately and will be reflected at the end of the line one after another and return toward the starting point. When a returning positive crest meets an advancing positive crest double potential will result at the point of meeting; similarly with two negative crests. When a positive and a negative crest meet, zero potential will result at the point of meeting. The result is the formation of nodes and loops in the line or cable, that is, points of zero and of double potential as shown in Fig. 6, where advancing waves are shown full, reflected waves dotted, resultant nodes and loops in dot and dash.

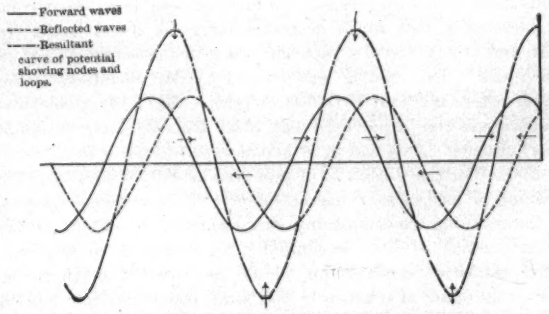

FIG. 6.—Nodes and Loops in a Long Line or Cable.

The waves sent into the line by the alternating e. m. f. are in continuous motion back and forward, but the nodes and loops are fixed in position though the loops vary in strength, alternating between positive and negative. This phenomenon is similar to the formation of nodes and loops in an organ pipe. There is always a loop or point of high potential at the reflecting point. The distance between two positive loops at any instant is the wave length of the moving waves.

To this point the lines and cables have been considered as open circuited at the farther end. Comparatively little difference will result in static effects, however, if they be closed through transformers at various points for the inductance of the transformer winding is so great that the comparatively large quantities of the charge on the line would not be materially lessened by the e. s. capacity of the transformers. In fact the transformer is sub-

jected to a very severe strain without its being able to relieve the line. This strain is similar to that produced by connecting a "dead" transformer to a live line, since the rise of potential of the tranformer terminal, due to reflecting the wave may be very abrupt.

Opening a High Tension Circuit.

When opening a load current in a high tension line or cable no rise of potential will result unless the current be suddenly interrupted. With such an interruption of course (as all commercial circuits contain considerable inductance) a rise of potential will result, its severity depending upon how suddenly the current is interrupted, how much magnetic energy is stored in the circuit, and how much e. s. capacity exists in the neighborhood of the break. This matter was treated by Mr. Steinmetz in his paper before the Institute last August. He there states that an excessive rise of potential may result from the interruption of short circuits. As a matter of actual experience it is the opinion of the author that little rise of potential actually occurs from the opening of load or short circuit currents in commercial systems, as the resulting arc cannot drop out suddenly on account of the great amount of heat generated. There are often, however, other causes of static strains which may result from short circuits, that occur at practically the same instant of time and in some cases the results of one cause may be assigned to the other.

However, by opening an unloaded line or cable, it is entirely possible to obtain a rise of potential as follows: On opening a switch to cut out an unloaded line the arc of the charging current tends to drop out when the current strength is zero, that is (since the unloaded line takes a leading current) when the voltage is a maximum; for as the line will momentarily hold its charge there is at this time little difference of potential between the line and bus bars, even after the switch is opened. The line is thus left charged when the switch is opened while the potential of the bus bars is changing with the generator e. m. f. When the voltage of the generator has passed through one alternation there will be a potential between the line and bus bars which may be sufficient to cause the arc to establish itself again and recharge the line. The arc will again drop out when the current becomes zero, and this action may be repeated several times before the line is finally clear. The sharp crackling sound often accom-

panying the switching out of a high tension line or cable suggests this phenomenon. This recharging of the line will cause the formation of a wave and the consequent rise of potential as in the case of charging a line. Thus, pulling off an unloaded line may have the same effect as charging it so far as the static rise of potential is concerned.

In the case where one terminal of the single-phase generator is grounded and charging current to a line is opened at the other a static wave may be produced in the line of double the intensity caused by charging the line "dead", that is, a wave of double line voltage, giving a maximum rise of potential of four times.

Other Causes of Static Strains.

In the previous discussion the author has endeavored to show that dangerous static effects result directly from very abrupt changes of potential. They may appear in parts of the circuit very remote from the exciting cause. These static strains are phenomena, such as would be expected to result from well known laws governing static electricity. In addition to the causes of abrupt changes of potential already discussed a few more will be briefly described.

(*a*) *Short Circuits.*—A short circuit between the two line wires when of a sudden nature causes an abrupt reduction to zero of the potential of one or both lines at the point of short circuit. This is the reverse of the case of charging a line. A wave is sent along the line causing a dropping of the potential, which of course has no tendency to *ground* apparatus. Nevertheless, this wave causes just as severe a strain on the insulation between turns of the transformer coils next to line as a charging wave for the potentials of the transformer terminals is abruptly reduced. Since the inner layers cannot get rid of their charge immediately there is a momentary potential across the coils between a high potential point in the inner layers and a low potential point at the terminal.

(*b*) *Grounds.*—Grounds are also a cause of abrupt change of potential and short circuits. The same strain on coils is produced as before, strongest on the side of the circuit where the ground occurs; also the ungrounded lines are raised to full potential above the earth. It is true that the rise of potential of the ungrounded lines is not necessarily as abrupt, since it is produced partly by the generator.

It must be remembered that the static waves produced in lines by grounds and short circuits may have their intensity doubled at a reflecting point, so that the short circuit strain on coils is doubly severe at such points. However, the strains to ground are not great in these cases except on the ungrounded wires.

It must be remembered that when a wave is caused by grounding or charging a line that the abrupt change of potential may sometimes be only the voltage existing between line and ground in that system and not the full line voltage.

In much of the previous discussion, transmission lines have been assumed to be single-phase. The conclusions arrived at are nearly all applicable to polyphase lines as well, with occasional changes which will be evident on inspection.

(*c*) *Lightning.*—Lightning is the best known cause of static disturbances. Unlike grounds, short circuits and switching, lightning usually does not act directly on the circuit but indirectly by induction. Its effects, however, are of the same nature as those already discussed.

In general lightning may affect a transmission line in two ways; by induction and by direct stroke.

(1) A lightning discharge in the neighborhood of an electric circuit but not actually reaching the circuit acts upon it indirectly by a combination of static and dynamic induction. The actual intensity of the effect in the circuit depends on the energy of the lightning discharge, on the distance to the circuit, its form and its position relative to the discharge path of the lightning and upon other conditions.

Lightning discharges are usually of an oscillating nature, that is, the charge on the cloud oscillates backward and forward between cloud and earth several times, pausing at each end of its path in each oscillation. This oscillating charge acts inductively on any electric circuit which is in the neighborhood, first, (static induction) when pausing at either end of its path (by attracting a charge of the opposite sign to the nearest point of the circuit) and second, (electromagnetic induction) since the actual discharge is a current and sets up an opposing e. m. f. in any parallel conductor. As the lightning charge appears first at one end and then the other of its discharge path it draws the charge on the line back and forth and the e. m. f. in the line induced by the current in the discharge is in such a direction as to accelerate the motion of the attracted charge. Therefore the effect

of the lightning discharge is to cause a static wave or a series of static waves in the line. These waves are similar in general character to those produced by switching, etc., except that there is no limit to their maximum possible strength. They may produce either grounds on windings or short circuits in coils as circumstances may determine. The static strains produced in a circuit by lightning have no direct relation to the generator voltage but are determined by the form and location of the line, the protective devices, and the nature of the lightning discharge.

(2) A lightning discharge may and occasionally does strike a line directly. In this case it is commonly supposed that nothing can save the apparatus from injury. This may or may not be the fact according to circumstances. If the discharge strikes the line at some distance from the apparatus, there is a very good chance that no harm may be done, for the choking effect of the line is enormous for the extremely abrupt discharge of a direct stroke. That is the discharge cannot pass down a long line quickly and instead will pile up voltage at the point where the line is struck and finally make some direct path to earth, usually by jumping to a pole. This relieves the line and no severe shock is sent to the station. The escaping discharge may or may not destroy the pole according to the nature of the pole and the intensity of the discharge. But although the chief discharge passes to earth directly there will be a wave sent along the line, which may or may not do damage according to circumstances; thus in this case the arresters are not required to discharge the direct stroke.

On the other hand, if the stroke is near the apparatus it is probable that protection will not be obtained from any lightning arrester. Even though the arrester were, for the moment, a direct connection between line and ground there would be sufficient resistance and inductance in the ground connection and the discharge path in the earth itself, to prevent an absolutely free discharge and the result would be that the lightning would find other paths to ground. The case of a direct stroke by lightning near a station very rarely occurs.

(*d*) *Paralleling out of Phase.*—One more case of static may be mentioned. When two lines are thrown in parallel out of step there is an abrupt change of potential produced when the second leg of the paralleling switch is closed. This voltage change may run up to double line voltage if the generators are directly opposite in phase. The same effects may be produced when two generators are running in parallel by opening all the

poles of the paralleling-switch except one, for the machines soon get out of step, and a discharge may pass between circuits—either over arresters or elsewhere. In this case a synchronous motor or rotary converter will act as a generator for a time long enough to cause any possible damage to be accomplished.

The most frequent causes of dangerous static disturbances in high tension circuits have been discussed. It is evident, however, that anything which causes an abrupt change of static potential of considerable severity may cause a dangerous strain on apparatus.

Resonance.—Resonance does not strictly speaking come under the head of "static," but to show its relation to the actions already discussed, some attention will be given it. As already frequently stated any condenser discharging through inductance (the resistance being small) has an oscillating discharge as a pendulum has an oscillating motion. If now an alternating e. m. f. be applied in the discharge circuit of the condenser in such a way that the e. m. f. alternates just as often as the condenser discharges and is always in the same direction as this discharge, the intensity of the oscillations will get greater and greater, just as the vibrations of the pendulum will get greater and greater if an alternating force is applied to it in such a way as always to increase its swing. There is no limit theoretically to the voltage that may be reached after a sufficient number of oscillations if there are no losses. However, in any actual trial the loss of energy in resistance and elsewhere gets greater as the oscillations get greater and will finally equal the energy put in by the alternating e. m. f. and thus stop the increase of voltage. Also, the exciting e. m. f. may slowly get out of step and so after building up the oscillations for a while reduce them again. The increase of voltage may be stopped by a break down of insulation which may occur on account of the high voltage. That is, if we have an oscillating circuit (as, for example, a short transmission line) and an exciting cause of exactly the right alternations a very high voltage may result. The exciting cause may be either the generator e. m. f. or it may be some static e. m. f.; for example, another discharging condenser. In very few cases in actual installations, however, has it been directly shown that resonance has caused any serious rise of potential. There is no justification for hastily assigning resonance as the cause for unexplained static phenomena.

The number of complete oscillations per second in a circuit consisting of a choke coil and condenser $= 1/2\,\pi\,\sqrt{L\,C}$. So for the circuit to have a frequency of 60 cycles would require that the product of its capacity and inductance shall equal $1/(2\,\pi\,60)^2$. If the capacity equals 10 microfarads the inductance must equal 7/10 henries, which is a large value. This shows that resonance with the generator e. m. f. is very unlikely in high tension lines. A condenser of 1/10 microfarad with a coil of 1/1000 of a henry has 16,000 complete oscillations per second.

When an alternating e. m. f. is applied to a long transmission line, however, the action is somewhat different from that of a simple condenser. The first alternation of the e. m. f. sends a wave down the line which is reflected back if the end is open. If the applied e. m. f. is just slow enough so that as it starts on its negative swing, the reflected wave is just beginning to arrive again at the starting point its e. m. f. is added to that of the impressed voltage, and a second wave of double strength is sent along the line—when the next alternation is reflected back the applied e. m. f. has reversed and again adds to the amplitude of the wave—thus just as with a simple condenser an excessive voltage will be built up if there are no losses. But suppose that the generator has three times this frequency. The applied e. m. f. will then have the same relative value, when the first wave reaches the beginning of the line after reflection, as when it has only one-third the frequency, though two other wave crests —one positive, one negative—have been sent into the line. The amplitude of the first wave will then be doubled as before and the same with the two succeeding waves. But by this time the first augmented wave has again been reflected and reached the starting point so that it has a further increase of amplitude. The same thing occurs if the applied e. m. f. has its frequency increased to five times its original value only five waves are operated on in a group instead of three as in the previous case; similarly with 7, 9, etc., times the original frequency (which was such that in the time of one alternation a wave would pass the length of the line and return). Resonance of this type with a long line is much more likely to be met than with a simple condenser, for there are a great number of frequencies at which resonance may occur.

If the periodicity of the applied e. m. f. is not exactly right for resonance (this applies to long and short lines) there will still

be a certain amount of rise of potential which will be less as the e. m. f. departs further from the proper frequency.

A summary of the conclusions so far reached follows:

(1) When a "dead" transformer is connected to a live line a strain may be produced on the layers of the coil next to the terminal which may be as great as the line voltage.

(2) When a short line is charged suddenly from live bus bars, a momentary voltage rise may be produced which will be not more than double voltage.

(3) When a long line is charged a strain similar to that in the short line is produced, doubling the potential first at the end of the line and afterward along the whole length. This result assumes that the charging terminal of the line is abruptly raised to full normal potential and rigidly maintained there until the wave is fully formed; also that the losses are zero.

(4) The fundamental principles governing the charging of a cable are the same as those of a transmission line.

(5) Opening a light high tension circuit may be nearly the equivalent of charging the circuit since before the line is completely freed from the bus bars it is often momentarily recharged several times.

(6) When a branch line of small e. s. capacity is supplied from a main line, the rise of potential at the farther end may be twice as great as at the end of the main line.

(7) In addition to switching, other causes of static strains are grounding, short circuit, lightning, etc., when these cause abrupt changes of potential.

(8) A rise of potential due to resonance is always possible, but generally improbable. It requires that there be an oscillating circuit and an exciting cause which must be of very nearly the same frequency as the oscillating circuit. When an alternating e. m. f. of high frequency is applied to a long line the danger that resonance will occur is usually greatest, as it may be produced by a large number of frequencies.

Part II.—Protection of Apparatus and the Static Interrupter.

Tracing of Damage Caused by Static.

From the discussion in the first part of this paper, the conclusion seems warranted that some sort of static protection for apparatus in high tension circuits is very desirable. This fact has,

indeed, long been almost universally recognized in a partial way as is shown by the wide use of the lightning arrester. But the lightning arrester as commonly found falls far short of affording complete protection, although it has, in fact, been quite effective in preventing grounds in apparatus (its only avowed object). As every person familiar with the operation of high tension electrical apparatus knows, short circuits as distinguished from grounds are a much more frequent cause of difficulty than grounds proper. Now, lightning arresters have little effectiveness for preventing short circuits, since the short circuit, when caused by static, is usually the result of the concentration on a small portion of the coil of a voltage, not necessarily great with respect to the line voltage, but perhaps very great with respect to the normal voltage of the portion of the coil where the failure occurs. Some protection, however, has frequently been secured by the use of choke coils in addition to the lightning arresters; but these have often been used in an aimless way, and have sometimes been omitted in very high tension circuits, where they are the most necessary.

The chief reason why static protection has not received more careful consideration (although the lightning arrester proper has been long in commercial use) is the very great difficulty of tracing the effects of static to their true cause. In the first place very little is generally known as to what causes static strains and as to whereabouts in the circuit they are to be expected, nor as to what sort of injuries they inflict. The direct damage from an excessive static strain is usually limited to the injury that can be caused by a spark carrying nothing but charging current to some small portion of the apparatus. Such an injury would be invisible to the eye. A great many repetitions would be required before an evident defect would appear.

In the second place when trouble develops from a static cause it is usually made manifest by an arc supported directly by the generator. This arc, of course, causes a great amount of burning of insulation and fusing of copper, which destroy all evidence of the original cause of the failure. Also at such a time usually little note is taken of attendant circumstances which could throw some light on the cause of the trouble. Most engineers will appreciate the great difficulty of getting afterwards the exact sequence of events in any breakdown. Again, sometimes a severe

static strain will select some particular point where some defect in material or workmanship exists, and this imperfection when found is assigned as the cause of the failure, even though the defective part may have been amply able to withstand normal potential.

Still another fact renders the tracing of static strains difficult. Only occasionally are the conditions favorable for the exciting spark to be followed by the normal e. m. f. of the circuit, which alone makes the spark detectable. That means that if a watch is kept for the effect of any particular cause of static, such as switching, perhaps nine times out of ten this effect will not be discovered, because it is not made evident by the generator e. m. f.

Sometimes a short circuit may be started by a static spark which involves so few turns of the coil or has so much resistance that the fuses and instruments will not be noticeably affected, and nothing is known of the trouble until it has developed, which may require some minutes or some hours.

I do not mean to give the impression here that I consider every failure of high tension apparatus to be, somehow or other, due to static causes, for such is far from the truth; nor that such difficulties are of everyday occurrence, for this would be much further from the truth; but it is true that static strains do cause breakdowns, and that such are very difficult to trace to their true causes.

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Protection from Static.

Protection from static is a question of growing importance, for several reasons. Circuits of high potential and long distances are rapidly extending, both as regards number, size, and potential. The troubles due to static are certain to be more commonly recognized. In the future the standard of service will gradually rise and nothing threatening its interruption will be tolerated.

I propose, in what follows, to describe a system of protection of high tension apparatus from static strains that is applicable to practically all cases of high tension transmission. In actual plants difficulties from static are found to occur usually in generators, transformers, or instruments connected directly to the high tension circuits, or between lines, or to ground at ends of lines or other reflecting points. Direct strokes of lightning are an exception to the statement regarding the location of damage.

Why strains should appear at these points has been already explained.

These injuries are of two distinct classes, short circuits and grounds, each type of injury being caused in a different way and requiring its own protective apparatus. They will be treated separately.

Protection Against Short Circuits.

As already fully explained, static causes short circuits by producing a momentary concentration of potential (which may or may not be greater than normal line potential) upon a few layers of the coils, which very probably may have been intended to withstand only a small part of the strain brought upon them. This excessive momentary strain results from a very abrupt change of potential of the terminal or other point of the coil, either up or down. With such a change, until charge can flow to the more distant layers, the full voltage of the potential change will be impressed on the layers of the coil between the point of application of strain and those points to which charge has not penetrated. If this voltage is too much for the strength of the insulation a spark passes. If conditions are favorable the e. m. f. of the generator causes a current to follow, and a burnout will usually result. In some cases, however, current from the generator may start to follow and drop out at the end of the first alternation.

The characteristic of the static wave which causes the static strain to be concentrated on a few layers of the coil is its abruptness. Therefore, if this wave be forced to pass through a choke coil and its abruptness thus reduced, the strain on the coil will be lessened; if the choke coil be sufficiently powerful the wave will be rendered harmless. It must be noted that it is by no means necessary for the choke coil to be effective, that it be made powerful enough to entirely remove the strain from the transformer coil; for the latter's insulation will withstand a much higher e. m. f. of a static nature which exists but an instant than of a continuous nature like that impressed by the generator. Also, there is usually allowed (perhaps on account of the results of previous experience with static) a very great margin in the insulation between the layers of coils.

The exact amount of abruptness allowable in the wave will depend on circumstances. Looking at the question more closely

it is seen that a choke coil cannot be said to have a definite protective power regardless of the apparatus it protects. For more choking effect is required to protect a transformer coil of many turns or layers than a coil of few. The condition to be fulfilled is that by the time maximum voltage is reached at the transformer terminal, charging current shall have penetrated far enough into the transformer coil so that a sufficient number of layers shall share the strain for their combined insulation strength to be able to resist it. The function of the choke coil is thus seen to be simply one of delaying the wave to give the inflowing charge time to become distributed over the coil. The length of time required depends on the rate at which the charge penetrates the coil and the strength of the insulation. The inductance of the protecting choke coil should be larger than that of the minimum portion of the transformer coil which is able to stand the strain of the most severe static wave that may occur.

But such a choke coil has other effects than the retarding of static waves, it will also retard the normal flow of current; in some cases to such an extent as to be quite an undesirable feature. The choke coil also has resistance and will waste energy, and unless a prohibitive cost of copper is used this loss of energy will sensibly affect the efficiency of the plant; further, the coil will get too hot. It will be found in most cases that with high tension apparatus choke coils powerful enough to protect perfectly are impracticable on account of size or cost.

Static Interrupter.

To meet this very serious difficulty the author proposes the use of a device he has called the "static interrupter." "The static interrupter" is a combination of a choke coil and a condenser connected in such a way as to magnify the effect of the choke coil as regards static waves without causing a sensible derangement the normal operation of the circuit. By the use of this device comparatively small coil can be made to afford the same protection as a coil of much larger dimensions without entailing either excessive cost or excessive loss of energy or heating.

The static interrupter consists of a choke coil in series with the line and a condenser connected by one terminal between the choke coil and the winding to be protected and by the other terminal to such parts of the apparatus as have e. s. capacity with this high tension winding; in a transformer to the low tension

winding and the core. However, as these parts are always (as compared with the high tension) at ground potential, the second condenser terminal may be connected directly to ground. The diagram of connections for the interrupter protecting a transformer or generator are shown in Fig. 7.

PROTECTIVE POWER OF STATIC INTERRUPTER.

It is evident that more time is required for sufficient charge to pass the choke coil to charge the transformer terminal and condenser together than would be required to charge the terminal alone. This means that the maximum static strain will be distributed over a larger number of layers in the first case than in

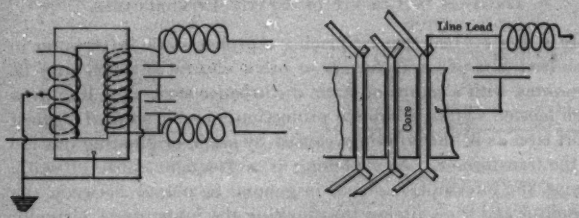


FIG. 7.—Static Interrupter Protecting a Transformer and High Tension Generator.

the second. The ratio of the e. s. capacity of the condenser to that of the transformer terminal and the adjacent parts of winding is an important factor in determining the protective power of the interrupter, as it indicates the extent to which the potential of the terminal at any instant is reduced by the addition of the condenser. It is possible to get almost any desired degree of protection by the use of a sufficiently large condenser without unduly increasing the size of the choke coil in series with the circuit. Actual experiment shows this method of protection to be very effective in reducing the static strains in coils.

On the other hand the use of the static interrupter although it delays the penetration of the crest of the advancing static wave does not necessarily prevent the ultimate rise of potential of the terminal, that is, it affords no direct protection against grounds. However, in cases where the interrupter is used in connection with a lightning arrester (which should always be connected on the side of the choke coil farthest from the condenser) the delay

of the wave does give additional protection against grounding of the winding as it allows the arrester more time to discharge the line.

The interrupter performs a more important function in connection with the discharge of an arrester by protecting the winding from the violent strains that result from the abrupt grounding of the circuit caused by the discharge. As the discharge of the arrester occurs only when line potential is much above normal and may be carried momentarily much beyond ground potential on account of its oscillatory nature, the shock to the coil may be a very severe one.

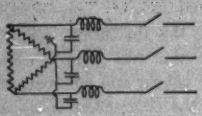
Location in Circuit of Static Interrupters.

It is thus evident that by placing a proper static interrupter in each lead wire of a transformer or other apparatus which may be connected with a source of static disturbance such as a line or a high tension switch, complete protection may be secured against short circuits in the windings caused by static originating outside of the transformer. As switching is a frequent cause of static strains, the interrupters should in general be placed between the apparatus and the switches thus making the interrupter virtually a part of the transformer or generator to be protected and not a part of the line. If, however, each individual transformer is provided with static interrupters a larger number of interrupters is required than would be necessary for protecting a bank of transformers as a whole from disturbances upon the transmission line. If there is no switching of individual transformers it is then satisfactory to arrange the apparatus so that several transformers will be protected by the same interrupters. For example, in polyphase systems a group may be protected as a unit instead of giving individual protection to each transformer. The precise arrangement of apparatus in a given case will depend upon the number of transformers to be protected and the arrangements which are provided for switching.

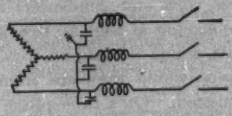
There are certain cases in which switching may be done between the interrupter and the transformer. The transformer may be cut out of circuit by the opening of circuit breakers, fuses or switches, or transformers may be connected to a live high tension circuit, provided their low tension windings are at the time supplied with current from the same generator as the circuits from

which they are switched. Such switching causes no change of potential at the terminals and consequently no strain.

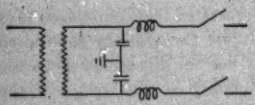
Also fuses and circuit breakers may be opened by overloads or short circuits without regard to the low tension connections or



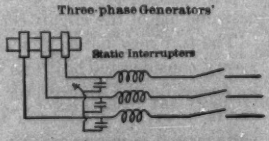
Three Static Interrupters Protecting Three Transformers Connected in Delta.



Three Static Interrupters Protecting Three Transformers Connected in Star.

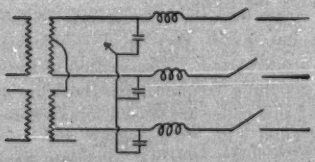


Two Static Interrupters Protecting One Transformer.



Three Single Pole Static Interrupters Protecting a Three-phase Generator.

Single-Phase and Two-Phase Generators are Protected by Two and Four Single Pole Static Interrupters Respectively, Connected in a Similar Manner.



Three Static Interrupters Protecting Two Transformers Connected for Three-phase High Tension and Two-Phase Low Tension Circuit.

FIG. 8.—Diagram Showing Arrangements of Static Interrupters Protecting Generators and Groups of Transformers.

without danger from static as the arc formed assures a gradual change of potential.

The proper connections for static interrupters in the arrangements of apparatus most frequently found are shown diagrammatically in Fig. 8.

Features of Commercial Static Interrupters.

Some of the features of actual static interrupters intended for commercial service work may be of interest. These interrupters are usually single pole; that is, one coil and one condenser are placed in a containing case. As the interrupters are used on high voltages, coils and condensers are placed in oil. The oil facilitates the cooling as well as strengthening the insulation. The choke coils, which contain no iron, receive the full force of the static waves which they are intended to check, and are very heavily insulated. On a three-phase circuit the condensers must support continuously a strain approximately 6/10 of the normal line voltage without breakdown and without overheating; they must also be able for moderate lengths of time to stand full line voltage in case one leg of the circuit becomes grounded.

The fact may not be generally appreciated that condensers have a very considerable heat loss which must be carefully considered in designing the interrupter. This loss for any given dielectric varies in actual condensers approximately as the square of the voltage, and increases rapidly with the temperature. The loss in a condenser may be increased several times by changing its temperature from 20° C. to 100° C. This fact indicates the importance of not allowing condensers to get hot.

The actual e. s. capacity of condensers for static interrupters is made much larger than the e. s. capacity of the windings they are to protect, but is still very small—seldom exceeding a few hundredths of a microfarad. The reaction of the condenser upon the circuit for generator frequencies is therefore negligible.

At the end of this paper are shown some views of a static interrupter for 50,000 volts, 19 amperes.

Protection Against Grounds.

A ground is the result of a rise of potential higher than the insulation of the apparatus is adapted to withstand (assuming that the insulating material contains no local defects). Such an excessive strain may result from static disturbances or from other causes. It has been shown that lightning may be one cause, and and that under certain circumstances switching and similar disturbances may produce high momentary voltages. Therefore unless the apparatus is insulated to stand such excessive potentials, which may be possible with low and moderately high voltage circuits, but is hardly practical with extra high tension, we

must use some means of prevention. The standard lightning arrester spark gap used to prevent excessive potentials from lightning is effective also against the other static causes.

Lightning Arresters.

A simple air gap between line and ground is an ideal lightning arrester or line discharger as far as its protecting power is concerned. It absolutely resists the passage of current up to a certain voltage which can be predetermined quite closely, and if this critical voltage is exceeded, breaks down and furnishes a non-inductive and nearly resistanceless discharge path for carrying off any amount of static electricity that may appear on the line. This means that when once the gap has broken down no amount of charge on the line can raise its potential above the ground. However, if the ground connection be not perfect or if either in the arrester or in its connections there is sufficient inductance or resistance, it is then possible to have a dangerous rise of potential on the line after the air gap has broken down due of course to the checking of the discharge of the line by the inductance or resistance.

Unfortunately, however, when single spark gaps are used as lightning arresters, their simultaneous discharge on two or more legs of a line forms a short circuit, and the usual result follows. If one leg of a circuit is accidentally or permanently grounded an arrester discharge on any one of the other legs causes a short circuit.

The air-gap lightning arrester to be commercially successful, in addition to giving protection from static must be able to interrupt the short circuit current which tends to follow lightning discharges. The devising of an arrester to meet all conditions is a very difficult problem. Fortunately, however, a large number of small gaps in series may be substituted for one single gap of larger dimension which greatly increases the "non-arcing" power. A very few gaps of non-arcing metal are satisfactory under some conditions; but the more the gaps are sub-divided the more effective the arrester. On high voltages, however, and under some other circumstances, such a large number of these gaps must be used on the arrester, as to interfere with its protective power by requiring a rather excessive voltage to cause the gaps to discharge. If the amount of the current that can flow on short circuit be limited either by series resistance or otherwise, a considerably less number of gaps will be non-arcing.

Series Resistance.

The use of series resistance to render an arrester non-arcing has the advantage of cheapness and simplicity, and is very effective as far as rendering the arrester non-arcing is concerned, but it has the great disadvantage of introducing the resistance in the path of the discharge of the line. In many plants the small size of the generators limits the current flow on short circuit in such a way as to render no series resistance necessary. It must be remembered in this connection, that all synchronous apparatus connected to an electric circuit (and sometimes induction motors) will temporarily act as generators at the time of a lightning discharge, on account of the energy stored in the momentum of their revolving parts.

In many cases where the short circuit current taken by the arrester is not excessively large the series resistance may be shunted by additional gaps which will allow the static discharge to pass directly to earth around the resistance. If the parts are properly proportioned, the subsequent arc will be drawn from the shunted gaps by the resistance and thus extinguished. If the number of shunted gaps is no greater than of the unshunted gaps no greater voltage will be required to discharge over the whole number than over the unshunted gaps alone. This is because the shunted and unshunted gaps are separately broken down—one after the other. Therefore no protecting power is lost by the addition of the shunted resistance.

In the cases where such resistance, shunted by gaps, is not sufficient a small series resistance in addition will make the arrester non-arcing. In other words where a large series resistance is required part of it may be shunted by gaps in such a way as to introduce only a small resistance in the discharge circuit of the arrester.

It should be noted, as in the case of short circuits in coils from static sparks that if the discharge through an arrester passes at or near the time of zero e. m. f. of the generator, there will be little likelihood of a permanent short circuit.

Although the insertion of resistance in the discharge path of an arrester may under some circumstances prevent the very thing that the arrester is supposed to accomplish nevertheless its great effectiveness in rendering the arrester non-arcing and its wide commercial use requires a statement of the conditions under which it may safely be used. It evidently makes no difference how much

resistance or inductance is used in series with an arrester provided it is always able to discharge the electricity as fast as it actually appears on the line at the point where the arrester is connected, without causing a potential in the arrester greater than that required to cause the arrester to discharge in the first place. In discharging electricity a resistance requires a potential equal to the product of the current strength and the resistance; inductance causes a counter e. m. f. equal to the product of its inductance and the rate of increase of the current. At each instant of time the current flowing in the arrester circuit has a definite value and a definite rate of increase (or decrease). Therefore for the arrester to meet the above condition the sum of the two potentials caused first by the current at the moment flowing in the resistance and second by the rate of increase of current in the inductance, must at no time exceed the potential required to cause the arrester to discharge.

It will be readily seen that the greater the quantity of static electricity to be discharged and the more sudden its appearance, the more severe the conditions on the arrester. Also that the greater the resistance and the inductance of the arrester circuit the greater the likelihood of a dangerous rise of potential. Just what the greatest allowable values of resistance and inductance are in any given case will depend upon various circumstances and cannot usually be definitely predicted. Therefore, in all cases the lowest practicable amount of resistance and inductance should be used in series with the discharge path of the arrester. It should be noted that with an increasing current flow through the arrester circuit the quantity of electricity discharged by the arrester before the dangerous point of potential is reached will be much more than proportionally greater with a low than with a high resistance. That is if the series resistance in one case is one-half that in another, both discharging the same increasing current, the quantity of electricity that will have passed the first arrester when a given potential is reached is much more than twice that passed in the second case when the same potential is reached. This fact makes it still more desirable that series resistances should be kept as low as possible.

Resistance and inductance in series with the arrester circuit have the advantage of preventing to some extent the very sudden grounding of the line that otherwise results from the arrester discharge and which causes severe strains in the windings of adjacent

apparatus. This is an advantage by no means to be overlooked, for it will be remembered that short circuits are much more frequent than grounds.

Location of Arresters.

A proper air gap lightning arrester will prevent an excessive rise of potential above the ground at that point of the line to which it is connected, but on account of the wave nature of the static disturbances it will not necessarily prevent an excessive rise of potential at some other point of the circuit more or less distant. For if the arrester happen to be at the zero point of a wave the arrester will not discharge, even though the crest of the wave at some other point may be at a very high potential above the earth.

It is a somewhat difficult problem to obtain complete protection at all points; however, in actual circuits adequate protection can usually be secured. Actual commercial plants fall into two classes, viz.: plants in which the apparatus connected to the high tension circuits is scattered about at numerous more or less widely separated points, and plants where the apparatus is all concentrated at a few points.

(*a*) Plants with Apparatus Distributed.

For absolutely complete protection from ground in plants having distributed apparatus arresters will have to be placed wherever there is apparatus that needs protection. In most cases this would mean an almost prohibitive expense. Fortunately such plants are ordinarily of comparatively low voltage, and therefore require protection (as far as static from grounds is concerned) only from lightning. Therefore, by placing arresters in such locations as to readily discharge static waves that may arise in the most exposed portions and by scattering arresters at intervals over the line at other points sufficient protection is secured. The exact number of arresters required in any given case depends upon the local conditions. This type of circuit, with distributed apparatus, is seldom found above 3,000 volts.

(*b*) Plants with Apparatus Concentrated at a Few Points.

In these cases the sources of static disturbance are either at some point away from the apparatus or are, electrically speaking, at the point where the line enters the station; that is, when a

rise of potential is caused by switching in the station all the high tension wiring is at the same potential and the rise can be limited by an arrester placed on each leg of the line at the point where the line enters. But these arresters, provided they be placed on the line side of apparatus, will also protect from dangerous static waves produced at points away on the line, for such waves must pass the arrester before they can reach the apparatus, and will thus cause the arrester to discharge. Therefore, in circuits of this type, which includes practically all high tension circuits, protection from grounds will be secured by proper arresters, placed one on each line wire where it leaves a station. The arrester must be placed outside of all choke coils and static interrupters, so that these may prevent static strain in the apparatus from the discharge of the arrester. This distribution of arresters is the one that has usually been followed in actual installations.

The system of protection outlined for high tension circuits therefore consists of the use of proper lightning arresters at appropriate points to protect against grounds, and static interrupters to protect apparatus against short circuits. The lightning arresters should be of adequate design, but no radical change from the usual present practice in regard to location is required.

The use of the static interrupter is, however, new, and it has been one of the objects of this paper to give a full exposition of the protection it affords and to discuss carefully the conditions which render its use advantageous.

Summary.

A summary of the most important conclusions regarding arresters and interrupters follows:

(1.) Damage resulting from static strains is of two sorts: short circuits and grounds It is the function of the static interrupter to protect apparatus against short circuit strains; it is the function of the lightning arrester to relieve the line of excessive static charge and thus prevent grounds.

(2.) The static interrupter consists of a choke coil and condenser, the latter being connected between line and ground on the apparatus side of the choke coil. The interrupter delays and smoothes out static waves which attempt to pass it. The condenser, by absorbing a large part of the static charge which passes the choke coil, very much increases the effectiveness of the coil.

(3) The choke coil and condenser of the interrupter must be adapted to the circuit upon which it is to be run, and must be proportioned with regard to the design of the apparatus it is to protect. The interrupter should be placed nearer the apparatus than any switches which are to be operated with potential on the line. (See page 224 for exceptions). The interrupter is thus essentially a part of the apparatus to be protected and not a part of the line or the arrester.

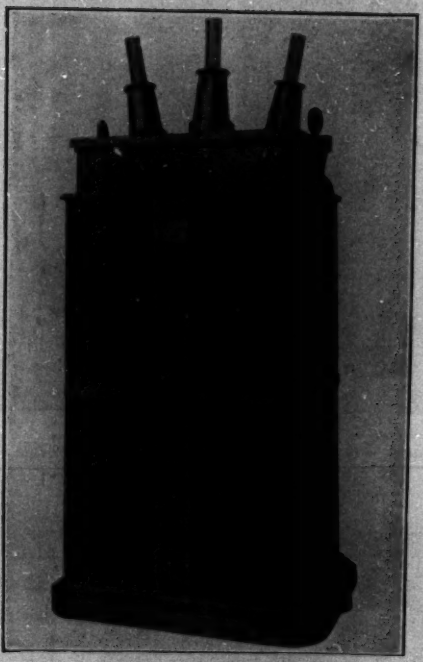
(4.) Lightning arresters must be able to discharge electricity from the line as fast as it actually appears there, and must also be non-arcing under all conditions. Any resistance or inductance used in series with the arrester should be low enough to allow the passage of all actual discharges without causing excessive potential on the arrester.

(5.) On the systems where the apparatus is distributed the arresters should also be distributed.

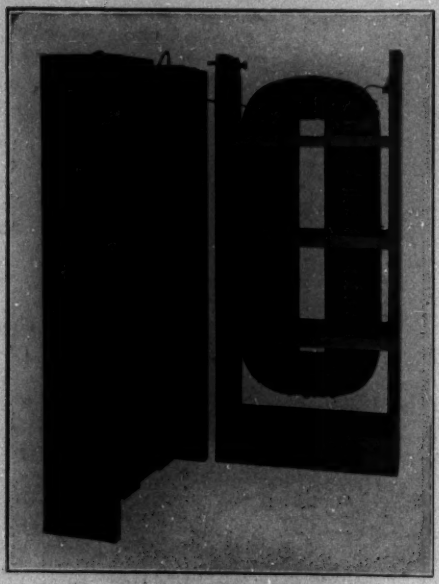
On systems with apparatus concentrated at a few points arresters should be placed on each line wire where it leaves a station.

(6.) Well designed static interrupters and lightning arresters, properly arranged, furnish a complete system of protection against the static disturbances found on high tension lines.

It is quite evident that the protection of the apparatus of a large high tension system, with its numerous opportunities for static disturbances and its widely spread apparatus, is by no means a simple problem, to be solved off-hand by the use of a few lightning arresters of any available style. The type and location of the protective devices must be adapted to the actual conditions of the plant;—the severity of the lightning in the neighborhood, the voltage of the system, the type of apparatus to be protected and perhaps the actual constants of the circuits. The determination of the most satisfactory protection is a matter to be arrived at from a combination of theory, experience, and experiment, and any protective system should be freely modified when actual trial shows a better arrangement. It is to be hoped that engineers and others who have the opportunity will carefully study experimentally the questions of static, giving them all the attention that their importance deserves, and that we may in consequence look forward to the time when difficulties from static causes may be practically unknown.



Static Interrupter with Case Complete.



A Static Interrupter Open.

Appendix I.

EXPERIMENTS ILLUSTRATING LOCAL CONCENTRATION OF POTENTIAL IN A HIGH-TENSION COIL.

Shown at the meeting of the Institute, February 14th, 1902.

To illustrate the effect of switching short-circuits and lightning in producing local concentration of potential in coils connected to a transmission line, experiments were performed as follows:

1. *Effect of Switching—Charging a Dead Transformer from a Live Line.*—The arrangement of the apparatus is shown in Fig. 9.

A 30 k. w. 30,000 volt raising-transformer was used to charge the line. The electrostatic capacity of the line is represented by a pair of oil condensers (*c. c.*) connected in series with their common terminal grounded. Each of these condensers had a

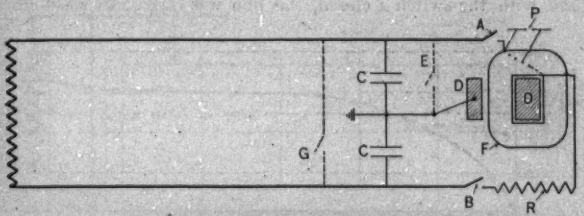


Fig. 9.—Diagram of connections to show effect of charging dead transformer and effect of short circuits.

capacity of approximately .012 microfarads. The ratio of transformation of the raising-transformer was such as to give approximately 25,000 volts on the line.

The transformer coil was a single coil of 52 layers surrounded by a heavily insulated metal plate D, which represented the low tension winding and the core. This coil was taken to be the coil of a lowering-transformer lying next the terminal at which switching was to be done. The remainder of the transformer, which is permanently connected to the other line, is replaced by the high resistance R, which serves to support the line voltage when the circuit is closed.

The blunt-nose spark-gap P was used to measure the maximum instantaneous voltage on different parts of the coil caused by switching. Leads were brought out at various points in the coil to facilitate this measurement.

In the experiment the switch A was closed and opened repeatedly until the measuring spark-gap could be so adjusted as to in-

dicate the maximum instantaneous voltage caused by closing the switch; this point was indicated when the gap would break down once or twice only in 50 trials. A large number of trials is always necessary, as the closing of the swing-switch does not always occur at the point of e. m. f. wave most favorable for causing a high potential.

By this means the maximum potential between the terminal and the 4th, 8th, 12th layers, etc., was successively determined. The results are plotted in a curve in Fig. 10. The normal voltage that would exist on a layer, were the coil in an actual transformer, is something like 75 volts. The maximum potential reached *between the lines* during switching was approximately 33,000 volts. This is a very considerable increase over the 25,000 volts of the line, and represents the rise of potential usually accompanying the charging of a condenser through inductance. It is evident that very great strains are produced by the switching on the layers next the terminal.

2. *Effect of Short Circuits.*—The same apparatus was used, and with the switch A closed, the line was repeatedly short-cir-

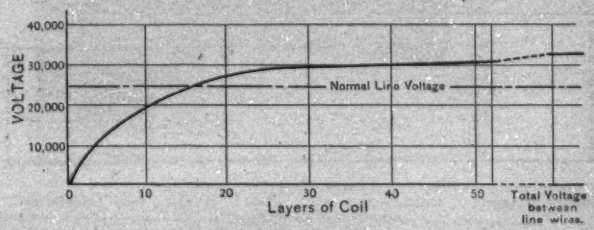


FIG. 10.—Curve showing concentration of potential on layers of coil.

cuited by a switch connected at G. Measurement of potential on the various layers of the winding, made in exactly the same manner as before, gave voltages approximately one-half of those due to switching, shown in Fig. 10. As the change of potential of the terminal in the case of short-circuiting is only one-half of that caused by charging the dead transformer, only half the instantaneous rise of potential is to be expected.

3. *Effect of Lightning.*—The apparatus of the test which is very similar to that used in the above experiments is shown in Fig. 11. The condenser represents the electrostatic capacity between a charged cloud and the line, and the closing of the switch H the discharge of the cloud. This arrangement is not strictly analogous to the effect of lightning on a line, as dynamic induction is not represented, but the effect of dynamic induction would be exactly similar to that of static induction, which is shown, so no error is introduced. Tests of the instantaneous voltages in the coil, as before, showed a curve practically identical with Fig. 10.

These tests show what the analysis in the paper requires, that abrupt static changes cause a local concentration of potential near the terminals of the coils, the amount of which depends simply upon the abruptness and severity of the potential change.

To illustrate the protective power of choke coils and static interrupters experiments were performed as follows:

1. *Choke Coils.*—Three choke coils were used, all having a mean length of turn about two feet. The first had 80 turns, the second 352 turns, the third 1,200 turns. With the coil of 80 turns placed in the lead of transformer coil next to switch A, the instantaneous voltage on the first four layers, which was caused by switching with no coil in the lead, was cut down about one-third. But this coil had no appreciable effect on the instantaneous voltage on the first 30 layers, showing that a few layers of the transformer coil may be much more easily protected than many layers.

With the coil of 352 turns connected in the lead, however, the voltage on the first 30 layers was cut down to nearly one quarter its original value. With the coil of 1,200 turns the abnormal volt-

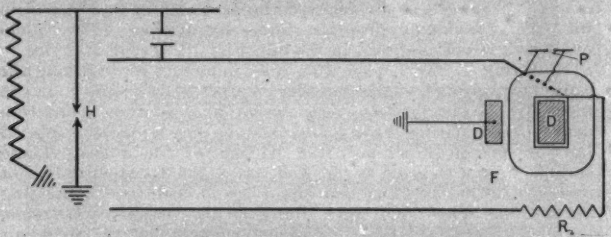


FIG. 11.—Diagram of connections to show effect of lightning.

age was cut down to less than one-tenth of its original value. Thus the effectiveness of a choke coil in reducing the short-circuit strains is clearly demonstrated.

2. *Static Interrupters.*—It had previously been shown, as described above, that the choke coil of 80 turns could not appreciably lower the instantaneous voltage on the first 30 layers of the transformer coil. When, however, a condenser of less than .0015 microfarads capacity was added, so as to form a static interrupter, this abnormal e. m. f. was reduced nearly 50 per cent., and with a larger condenser was cut down to approximately one-quarter of its original value. A still larger condenser would have practically removed the abnormal potential altogether. Or if the voltage in the first 15 layers had been measured, instead of in the first 30, it would have been found a few per cent. of its original value.

It is possible by properly proportioning coil and condenser to get any desired reduction in the abnormal voltage on any desired number of layers.

Appendix II.

MECHANICAL MODEL ILLUSTRATING THE PROPAGATION OF STATIC WAVES IN A LONG TRANSMISSION LINE.

Exhibited at the meeting of the Institute, February 14, 1902.

To illustrate the propagation of static waves in a long transmission line, such as result from abrupt changes of static potential, the mechanical model described below was exhibited. A long transmission line (or cable) may for all practical purposes be represented by a series of choke coils and condensers as in Fig 1, page 195. The law governing the currents, charges and e. m. f.'s in this circuit may be stated as follows: At any instant the difference in potential between any two neighboring points is equal to the e. m. f. consumed by the passage of the current through the resistance of that portion of the line wire between the points, added to that consumed in the back e. m. f. due to the inductance of the line between the points. If any mechanical device can be found, such that the same law will govern the motion of its parts, then exactly the same sort of waves will be produced in one case as in the other, provided appropriate forces are applied. The model exhibited, which is shown in the cut, Fig. 12, is such a device.

Referring to Fig. 1, page 195, the condensers representing the capacity to ground at the points B, C, D, etc., are replaced in the model by the springs seen near the top of the frame. The condensers representing the capacity between line wires are replaced by the springs shown near the bottom of the model. These springs are not so large as the first, as so much extension is not required. There are two sets of springs of both sizes, indicating the two line wires; one set is hung on the front of the frame, the other on the rear. The inductance between two adjacent points of the line is represented by the inertia of the lower line of pulleys, which are controlled by the small springs. These wheels are made thick to give them mass. The springs representing capacity and the heavy wheels representing inductance are connected together by an endless thread which serves the function of a conductor between points. As this thread must move freely it is carried on pulleys; it must not slip, however, on the heavy pulleys. The resistance of the line is represented by the friction of the wheels and the stiffness of the thread. It must be noted here that the model is not strictly analogous to the electric circuit in this respect, for its friction is not strictly proportional to the velocity of motion, as it should be, since the loss of e. m. f. due to resistance is exactly proportional to the current. However, as the friction is relatively small the movements of the model are almost wholly controlled by the springs and the inertia; the resistance simply causes a dying out of the waves.

The two divisions (front and back) of the model representing

the two line wires are connected together through the heavy wheels at the bottom, which are rigidly linked in pairs by cords running over stationary pulleys. A motion upward of one heavy wheel means an equal motion downward of its mate. That is, equal positive and negative charges must appear simultaneously in the two transmission wires. As the lower springs represent the capacity between the line wires, their extension measures that part of the total line charge due to this capacity, and at the same time indicates the voltage between lines. Similarly, an extension of the upper springs indicates that portion of the total line charge due to the capacity to ground, and, as well, the

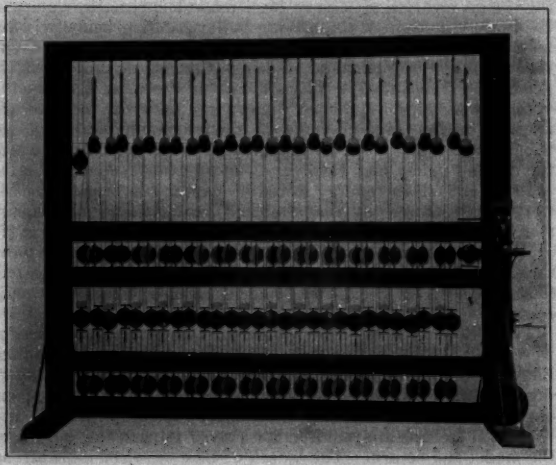


FIG. 12.—Mechanical Model of Transmission Line.

potential rise above the earth. Motion or displacement of the thread which runs over all the pulleys indicates quantity of electricity. Velocity of motion represents current strength. Force or tension on the cord represents e. m. f. and the device used for producing this force, the generator. The cord runs from one line to the other through the generator, so that whatever current flows out at one terminal must flow in at the other. To represent the lines short circuited at the farther end, the cord passes freely from one line to the other. To represent an open circuit in the line at any point, it is necessary only to hold the thread rigidly at the point where the open circuit is supposed to exist,

so that no electricity can pass. Then, releasing the thread is the equivalent of closing the open circuit.

The generator consists of a pulley carrying the endless thread supported by two loops of cord running in proper grooved wheels, as shown at the right in Fig. 12. These loops or belts hang on a revolving rod driven at a high rate of speed by a motor, so that they slip continuously on the rod. The pulley and grooved wheels, which are fastened rigidly to the same shaft, are free to turn by the slipping of the belt. The friction of the belts on the revolving rod causes a constant torque on the pulley carrying the thread, and consequently on the adjacent parts of the line. This torque acts as an e. m. f. As the rod supporting the belts revolves at a very high rate of speed, the comparatively slow motion of the driving pulley hanging below will not materially affect the friction. Therefore the e. m. f. is constant regardless of the "current" flowing (that is, regardless of the velocity of the thread). The amount of the e. m. f. applied is varied by hanging weights on the driving pulley to increase the friction of the slipping belts on the supporting rod. These weights must, of course, be supported through bearings in which the driving pulley may revolve.

In the model, the energy stored in capacity is represented by the energy stored in the stretched springs; and the energy stored in the inductance is represented by the kinetic energy in of the revolving pulleys. When a steady direct current flows through the line storing magnetic energy in its inductance, all the wheels have a constant velocity and a constant kinetic energy.

The following well-known phenomena in a transmission line were illustrated by the model: Charging a dead open-circuited line; short-circuiting a charged line at the farther end; opening a short-circuit at either end; ohmic drop in the line; energy stored by direct current in the short-circuited line.

Several seconds are required for a wave to pass the length of the model and return. The length of time required depends on the moment of inertia of the wheels and the elasticity of the wheels. Each heavy wheel weighs six ounces and is approximately $5\frac{1}{2}''$ in diameter. Each of the large springs has such an elasticity as to extend $26''$ under the weight of one-half pound. The small springs extend $10''$ under the same strain. It is important that the friction be made as small as possible.

Discussion of Mr. Stillwell's Paper at Buffalo Meeting.

[See p. 541, August and September issue.]

[In concluding his remarks, Mr. Stillwell invited discussion on the subject of interruptions of circuit, and on that topic spoke as follows:]

Mr Stillwell.—Several obvious improvements can be named. Among those that have been thought of is improvement in the regulation of the alternators. This will be provided for in the new machines, which I presume will be used ultimately for long distance service, while the older machines will be used for the local service. Another thing that has been tried is the time-limit circuit-breaker. We found by experience that fuses and circuit-breakers without some relay device that was adjustable were not discriminating, that a short circuit on a branch or a sub-station would interrupt the whole supply to the city. To get around this difficulty we have endeavored to use a time-limit device, setting the circuit-breakers on the branches from sub-stations to open instantly and those at the terminal house connected into the cables which supply the sub-station to open, say, in one second, and those at Niagara controlling the transmission circuits to open in three seconds.

This plan, if the circuit-breakers work as expected, is a discriminating arrangement. If a short circuit occurs in a circuit distributing energy from a sub-station, the circuit breaker which controls that feeder cuts it off in a small fraction of a second, the time-limit mechanisms of circuit-breakers at the terminal house and at Niagara start, but stop again the instant the circuit-breaker on the distributing feeder operates, and consequently the circuit-breakers at the terminal house and at Niagara do not open. If for any reason the circuit-breaker on the distributing circuit from the sub-station fails to clear, the circuit-breakers at the terminal house, controlling cables which supply power to the sub-stations will open at the end of one second. The circuit-breakers at Niagara can open only in case the overload due to short circuit persists for three seconds.

This arrangement has worked in some cases, but the circuit-breaker problem, so far as our experience has indicated, has not yet been solved satisfactorily. Of course, it is a very difficult problem, as Mr. Steinmetz said yesterday, to deal with these very large amounts of power, comparable with the energy of the Empire State Express going at 60 miles an hour, but that is a direction in which we must make progress. We must get a circuit-breaker which will operate instantaneously, or as nearly so as possible, so far as the making and breaking parts are concerned. Then I think we must use a time relay device to discriminate, so as to localize our difficulties. Improvement in the regulation of the machines will help. Whether we can improve matters further by using more induction motors and not so many synchronous motors is a question that is certainly debatable.

The converter in the case of a short circuit tends to protect itself and to hold on to a certain extent, because its load drops automatically when the potential supplying the converter drops. In our experience with synchronous motors, however, and with rotary converters here in Buffalo, at the end of a long line, where it is not only a question of drop in the alternator, but drop in the transmission circuits and in the transformers, they have always let go if the short circuit has been at all serious. I have felt that perhaps the induction motors would not let go. A drop of potential lasting perhaps a second or two might not cause the induction motors to stop, whereas it inevitably does so far as our experience goes, stop all synchronous machinery.

PRESIDENT STEINMETZ.—Gentlemen, I am sure you agree with me that we have listened to a very interesting paper, one in which Mr. Stillwell has brought before us the first time I think it has ever been done, a general review and description of the enormous work which has been done here in developing power at the largest of all the waterfalls in the world. In opening the discussion I should like to ask two questions of Mr. Stillwell for our information, one, referring to the wave shape of the alternators, the other, the efficiency. Now, I understand that the wave shape given in the paper has been taken with the machine in the factory revolving at very low speed. I believe it would be interesting to hear whether the wave shapes have ever been taken with the machine running under service conditions, that is at full voltage, full excitation, driven by the turbine at no load, and also at full load, perhaps also with loads of different character. The machines are of rather high armature reaction, and frequently the wave shape changes with the load in such machines.

Then, another point I think we should like to get information upon is regarding the efficiencies of the alternators given here in the paper. These electrical efficiencies, 98.1 per cent., are calculated by adding the losses. In the loss, however, the load loss is not counted. Now, it would be interesting to hear whether any tests have been made to determine these load losses of the alternators, and how much they amount to and what effects they have on the efficiencies. In alternators of high armature reaction the load losses are occasionally quite considerable.

I believe Mr. Stillwell will be very glad to discuss the various problems referred to in the paper with you and I hope we shall have a very interesting discussion.

MR. STILLWELL.—As regards the form of potential curve of the alternator, the result of the factory tests has been checked by an elaborate series of observations at the Falls which were conducted by Mr. Tingley of the Westinghouse Company, who may be present. These tests were made under varying conditions of load. Under non-inductive load and upon open circuit the curves agree very well with those obtained in the factory,

under the conditions of a very low speed. For varying loads the wave was very considerably distorted.

In regard to the efficiencies that are set forth in the tables, the iron loss was carefully determined by driving the machine by direct current motor on open circuit. We have added to that iron loss 20 per cent. as the assumed increase due to distortion of the field in the armature teeth, and this amount is included in these losses which are charged against the machine in the table that I have given.

MR. BUCK.—MR. President, this paper is so complete that there is little to add or to criticize in connection with it. The historical part is certainly interesting, for the reason that the history of the Niagara Falls Power Company has to a large extent been the history of electrical engineering during the past ten years, and it is also interesting as bringing before us to day the engineering point of view which existed ten years ago. It seems to me that electrical engineering owes a considerable debt to the Niagara Falls Power Company. Ten years ago electrical engineering was in somewhat of a chaotic condition. Few engineers agreed upon systems, and they were afraid of the alternating current. Polyphase systems were looked upon with considerable skepticism, and men with capital, who could be persuaded to put their money into such enterprises, considered the transmission of large amounts of power as being more of a theoretical than a practical proposition. Engineering at that time needed a large installation, similar to that at Niagara Falls to establish confidence and to serve as a precedent for engineering practice. The Niagara Falls Power Company adopted a frequency of 25 cycles. This was a very radical and important step, for at that time the prevailing frequency was 125 cycles and 133 cycles, and it was looked upon as necessary by engineers. The prevailing alternating generator voltage was 1100, and the Niagara Falls Company raised it to 2200, which has since been adopted as the standard for a great number of installations I do not recall the figures for the largest generators which had been built at that time, but they were comparatively small. The Niagara Falls Power Company jumped to a size of 5,000 h. p., demonstrating very forcibly that the large size units were practicable and economical. It also demonstrated the feasibility of transmitting enormous amounts of power, enormous in comparison with anything which had been transmitted up to that time. The system remained practically a standard since the time when it was proposed, which I believe was 1892, a very remarkable record.

MR. LINCOLN.—MR. President, Mr. Stillwell has called attention in his paper to the method of delivering power to the Union Carbide Company, and in connection with that has called attention to the heat losses in conduits. What he says there about the installation of rubber cables in connection with the great loss of heat is something which requires considerable thought. He

has called attention to the fact that the rubber deteriorates by heat continuously. There is, however, one other condition to be noted at Niagara Falls which also seems to be pretty hard on rubber cables, and that is where the rubber is subject to constant static strain. To state a concrete case: at the terminal of our 20000-volt lines inside the transformer house of the Niagara Falls Power Company we installed rubber cables having $\frac{3}{32}$ inch rubber on the outside and where it ran down the brick walls it was supported in porcelain clamp insulators, so that any leakage current had to go over the surface of about $\frac{3}{4}$ inch of porcelain as well as through $\frac{3}{32}$ inch of rubber before it reached the iron clamp holding the porcelain. A test of the rubber cable alone showed it was capable of standing at least 70,000 volts. I should judge that the insulation of the cable, plus the porcelain insulator as it was put in, would stand 100,000 volts. But that same insulation after a week's service, broke down under the normal strain of about 13,000 volts between line and ground. It has occurred to me that possibly this deterioration might be caused by the ozone which is generated by static strain, the ozone attacking the rubber. Of course, the experience I have had does not prove the fact conclusively, but it points in that direction, and I thought it might be interesting to refer to the members for discussion.

MR. SPERRY.—Mr. President, there is only a word I have to say and that is in reference to ozone. We are working with quite large amounts of ozone, and it will certainly attack rubber. A hose made of the best Para rubber, having, I think, a $\frac{3}{8}$ wall, will stand only about a day and a half when conducting atmospheric air charged with only $1\frac{1}{2}$ per cent. of ozone. So, doubtless, it is the ozone that we must look out for in rubber insulation where static effect is present, especially where the tension is high enough to cause silent discharge.

MR. PERRINE.—Mr. Stillwell has very properly called attention to the difficulty and the practical unreliability of temperature measurements either by resistance or by thermometer. With the thermometer we get a local temperature that may be correct, and with resistance we get an average temperature that is more likely to be correct, as an average. I have not the slightest doubt that our rules are in error in their form. I believe that the proper method in specifying for apparatus should take into account the highest temperature obtainable with the thermometer and the average temperature obtainable by resistance, and in any particular apparatus these should not differ more than a certain percentage. If we have a high, or even a very low, temperature ascertained locally by a thermometer, and a moderate temperature ascertained by the resistance, we know that the thermometer in these cases indicates that there are certain other points which are greatly different from the average, and I believe that this should be taken into account in our rules. I

am very glad that Mr. Stillwell has called attention to this point at this time.

As regards insulator-pins—the absolute stupidity of always using Western Union pins was called to my attention more particularly in one case where one of our prominent manufacturing companies was installing a plant for high voltage. They purchased a lot of large insulators and a quantity of Western Union pins, and after the material was all bought and on the ground they found that the petticoats of all the insulators rested on the cross-arm after they were screwed down to the pin. Then for the first time they saw that it was necessary, sometimes at any rate, to design pins.

As regards the matter of protection against interference with long distance lines, which Mr. Stillwell has called to our particular attention, asking for comments. I do not know whether their principal trouble here is due to normal occurrences, such as short circuits on the line, and failure of machines and other apparatus or to intentional malicious interference. As regards the normal troubles, I think that much remains to be worked out in respect of machinery. We have perhaps heard enough about synchronous and induction motors, but I am declined to think that a more correct design of synchronous motor will hold on better than the motors commonly used. But that was not the point about which I wished to speak. In the experience that I have had, I have found that disturbances to long distance lines due to abnormal difficulties—that is malicious interference with the line, can best be overcome by the systematic and general use of the newspapers, inculcating in the minds of the people living along the line the knowledge that inteference with the line means not so much damage to machinery and harm to the hateful millionaire who has built the line, but danger to the operators in the station. We have for the protection of some of our long distance lines inserted articles from time to time in local papers, in which we called shooting at insulators and throwing bale-wire over the lines intentional homicide if not cold-blooded murder; and by judiciously distributing such information we have found that there is actually a very material lessening of malicious disturbance.

I do not believe that Mr. Stillwell is correct in his assumption that the non-collection of sleet on the Niagara line is due to the static effect. I believe that it is almost entirely due to the heat effect, and the reason I have for that belief is that a number of years ago I observed a small line carrying current for a Thomson welding machine, passing over the roof of a rolling mill and through the steam from a non-condensing engine. The current in that line never amounted to enough to heat theline, but the potential was always present to a sufficient amount to make the static effect very marked, with the result that the water was, to be sure, repelled from the line, but the temperature of the wire not

being sufficient to prevent the formation of ice, we had icicles from two to three inches long standing up in all directions around the wire. I have seen icicles three inches long standing straight up. And so, while the water is repelled, I believe that heat is the essential for the non-collection of sleet.

MR. STERN.—I would like to ask Mr. Stillwell if they have had any of the experiences with anchor ice, such as the holding back of the water supply for the penstocks, thus cutting off the power and minimizing the output to any extent during the spring of the year.

MR. STILLWELL.—As regards anchor ice, Mr. President, there is a great deal of it encountered at Niagara at certain times. Some of the hydraulic "doctors" have told us that we ought to cease operations under those conditions, but we found that impracticable. Consequently we have done something that none of them has approved, but which seems to be effective—we have simply lifted the racks and let the anchor ice go through the wheels.

There is only one thing further I should like to say bearing upon the discussion, and that is in regard to operation, and to answer a single question suggested by Dr. Perrine as to the classification of the causes of trouble. I have not the classification here—perhaps Mr. Barton might supply if he is present—but I did make a rough approximate classification awhile ago of the interruptions, and from recollection I think this was about the way it worked out. Interruptions from cables were responsible for about 30 per cent. of our total troubles, interruptions from failure of exposed contacts at switches, circuit breakers and elsewhere, for another 30 per cent. or thereabouts. These two put together make 60 per cent. and these we may hope reasonably to eliminate as the weak parts are broken down and removed. I wish to say that in doubling the potential recently from 11,000 to 22,000 volts the company naturally had very much more trouble than in the two years just proceding the change. These incidental troubles I expect will all gradually be eliminated. Errors in throwing switches by operators at Niagara and elsewhere on the system—and practically none of those was made at the power house, where we can have highly skilled operators,—caused 10 per cent. Where, as I said yesterday, you have 24 $50-a-month men throwing switches and attempting to harmonize synchronous machinery, you are likely to have at least 10 per cent. of your interruptions from that source. Outside interference caused about 10 per cent., and lightning about 20 per cent. Now, this is an unusually high proportion of damage by lightning. There was one storm in which the line was struck five times. I think five poles with their cross-arms were destroyed. There is nothing to do when this happens but to let go and start up again. Such an occurrence is abnormal, however. The troubles from the cables we may hope to decrease

and upon that point I hope we may be able to gain some advice from our president and from other gentlemen who are pursuing the theoretical study of the harmonics, which I believe are responsible for some of our troubles; and we can also decrease them by strengthening our insulation, as experience shows us where it is weak. As to errors in switching, I do not believe it is possible to reduce them materially. I do not believe there is any operating force in existence better disciplined or more skilled than the one employed by the Niagara Falls Power Company.

As to outside interference, no attempt has been made to decrease it by the method suggested by Dr. Perrine, viz.: advertisement of the danger to operatives involved by such interference. We have tried almost every means, including detectives and patrols and all that sort of thing. The only cause of trouble that we cannot practically get away from until we put down an underground system of cables from the Falls, is probably that from lightning, and the point where I think we can make the greatest gain is in keeping our potential wave through the whole system as near a sine as practicable.

PRESIDENT STEINMETZ.—Gentlemen, we have with us Dr. Perrine, who I know has given the problem of lightning protection on transmission lines, and the study of transmission lines altogether, a good deal of attention. I think we would be interested to hear from him his experience with lightning protection. I call upon Dr. Perrine.

DR. PERRINE.—I think, Mr. President, this is taking rather a mean advantage of me, because, riding down on the car Mr. Steinmetz asked what my experience was, and I told him comparatively little. My experience with lightning protection is really comparatively little. On the Pacific coast we have lots of long lines, and it is there that my experience has lain. While we do not have the severe storms that you have here we have the unusual record in California that there is no month of the year entirely free from lightning. The occurrences that Mr. Stillwell describes of lightning storms being so severe as actually to strike the poles, we have very little difficulty with, but our lines being long,—a ten mile is a very short one there, and they run on up to 25, 30, 40, 50, 80 miles, and two recent lines have been built, one of 144 and the other one of 54 miles —which two lines at present are partly connected together in operation from one of the stations, making a total transmission of 198 miles,—with such work as that you can realize the fact that while we do not have the difficulty that you have here of the severe storms being localized along the lines and the liability of striking poles, at the same time we do have great trouble from the induction on the line, due to lightning storms in the neighborhood, because a lightning storm in the neighborhood of a 200-mile line is not an infrequent thing; and for that

purpose we use the same protection that you do here, of choke coils and lightning arresters between them. Mr. Steinmetz asked me whether I thought the choke coils really had any good effect. I told him that the only good effect that I really knew they had, was that they induced the owner of the line to buy more lightning arresters, because if you don't put in choke coils and indicate to him that more lightning arresters than a single set are necessary, he will say, "Well, I will buy one set, but that is all I will buy;" and we know that one set is never sufficient, whether a choke coil is in or not. In these long lines we have used some sort of guard line, generally a barbed wire thoroughly grounded at least as frequently as every fourth pole. The method of doing that is to put an iron plate under the bottom of the pole, and connect that with the barbed wire stapled to the top of the pole. The effect of that wire seems to be be to keep the line continuously discharged. In very dry air on long lines, where the elevation of the line varies all the way from practically sea level to 1500 to 2000 feet above the sea you will find severe charges on the line in almost all weathers, whether there is any lightning in the neighborhood or not, unless that line is discharged by means of some sort of spark gap or this barbed wire. If you have barbed wire along the line you can handle the line with practical impunity in almost all weather, that is, I mean, the idle line. If you have no barbed wire along such a line you will get knocked off your feet every time you touch the wire, even when there is no generating apparatus connected with it at either end; and while that constant potential, due to the dry winds blowing over the line and charging it with frictional electricity, may not be very great, it is nevertheless a potential high enough gradually, in time, to puncture the insulation of the machines; and now whenever there is a lightning discharge in the neighborhood of the line, and there is disturbance set up along the line, that barbed wire discharges it. Mr. Steinmetz asked me whether I thought smooth wire, being more reliable as regards its strength, would not be just as good as the barbed wire. I do not believe it would, because the barbed wire undoubtedly discharges the line when it is charged, as I say, due to the air moving over its surface. It is true that the barbed wire discharges a great space of air, and the fact that that line is dead when the barbed wire is strung over the pole might be due to the fact that the barbed wire discharges the line itself as well as the air, after the line has become charged. Lines protected in that way, with the lightning that we have had, are practically free from disturbance. There was one case of an exceedingly severe storm that swept north and south through our State, across our line, which was extending east and west, and our line, 46 miles in length, was the only line, telegraph, telephone or power line, in that neighborhood which was not interfered with; and during that storm my assistant

watched the lightning arresters very carefully, expecting to see a great deal of fire works. He sat out on the roof of the transformer sub-station where the lightning arresters were nearly all one night waiting to see the fire works, and didn't see any. Nearly all the lines in the neighborhood were incapacitated, and I believe that in spite of the acknowledged disadvantages, and they are altogether mechanical disadvantages, there is no lightning protection we can give that is so good as the almost continuous lightning rod afforded by such a barbed wire. Now, when you have such occurrences as Mr. Stillwell describes, of an actual lightning flash striking a line, I am reminded of what our President said a number of years ago, in discussing, I think, a paper of Mr. Wurts. He said, "There are three kinds of lightning that you have to look out for. First, a lightning discharge in the neighborhood of the line, which your lightning arresters generally get rid of; second, a secondary flash that strikes the line, and that your lightning arresters will probably get rid of, though probably the lightning arresters will be destroyed; and, third, a direct flash which strikes the line, and then you have to trust in Providence and repair what is left of the station."

MR. STOTT.—Mr. President, in connection with this very interesting, and one of the most practical, papers we have had, a point comes up that it seems to me would justify a good deal of investigation, and that is the fact of resonance being established. Apparently on the Buffalo transmission line now there are times when such conditions obtain, I have been informed, that with a ground on one of the lines arcs will be established between metallic points ten inches apart. Apparently, that is not the e. m. f. of a resonance which could come up to 100,000 volts or more. I know our President has been taking up the question of resonance very closely, and I think it would be very interesting to us to hear what his opinion is on these effects.

PRESIDENT STEINMETZ.—Gentlemen, it is very difficult to say what is the cause of such high voltages appearing in lines which give strokes of a foot or so, because there are quite a number of causes; but, knowing that there is some interference, malicious or otherwise, on the system, I should be suspicious of this interference. It may be some accidental short circuit of the line, some boys throwing wire over, or some machine suddenly changing the load, or any other circumstance which abruptly changes the conditions existing in the circuit which gives rise to a resonance voltage practically unlimited, that is, limited only by the discharging distance from a charged wire to the next ground or return circuit; but the most prominent source of disturbance I should suspect would be an accidental short circuit, or it may be even a moderately high resistance short circuit, so that the current consumed by the short circuit would not be noticeable in the power house in a system of such enormous magnitude.

MR. CLARENCE E. GIFFORD.—In connection with the visit of

August 22d, which included one of the railway transformer stations, it is possible that an account of some difficulties at first experienced in these stations in regulating, also in starting rotaries after a shut down at times of heavy load, and the manner in which some of these difficulties were overcome may be of interest. It should be here noted that these interruptions generally came from causes outside the station, although at first they occurred frequently from the relay which trips the main line circuit-breakers being set for too small a load.

The rotaries are 400 k. w. three phase, the armature being single wound provided with contact rings at one end and commutator at the other, as was probably observed.

The compounding, which is practically perfect, is for constant potential at the machine, and the d. c. voltage is about 600 when current is being received by the static transformer at 10,500 volts, and 25 cycles. At a higher periodicity and consequent increased speed of the rotary, the d. c. voltage is higher in proportion to the main line or primary a. c. voltage, for evident reasons.

There have been numerous times, especially in the winter, when, during the middle of the night, the speed of the generators at the Falls was so increased as to "make Rome howl" at the transformer stations. The a. c. voltage would be generously kept up to 12,000 and sometimes rise to 13,000. Of necessity, on account of lamps at that time used in the cars—five 110 volt lamps in each series,—the rotary fields had to be weakened to the full extent of the rheostat, still giving a d. c. voltage of 600 to 625, with the car lamps lighting up like small arcs. The a. c. ammeter reading was, of course, disproportionate to the output of the rotary. In this current, the wattless component (so called) was certainly very greatly in excess of the power component, but viewed with regard to heating effects, phase displacement is plainly without influence.

So far as the attendants of the railway power stations are concerned their ability has been somewhat underrated, and the difficulties with which they have to contend have not been mentioned. Salaries as small as $52.50 are paid in this vicinity, but most of the rotary transformer attendants receive from $72 to $81 per month, which prices, although not especially attractive, yet permit men of fair standing in the profession to obtain practical experience in the operation of such machinery without any serious privation financially; and this has in some cases been done in this city.

From a preceding paragraph it will be seen that there are times when the proper regulation of the power factor is forced out of the problem by other considerations which are, for the time, imperative.

The "horse sense," however, of almost any careful attendant, has been found to cause him ordinarily to give such field regu-

lation as results in the smallest reading of the a. c. ammeter. If he is ignorant of the effects of phase displacement he attaches even greater importance to proportionately excessive readings of the a. c. ammeter.

Rotaries are started as d. c. motors synchronized and thrown on to the power line. The method of starting by throwing directly on to the power line, the field circuit being open at starting and then closed after the machine is up the speed, should not be considered where other means are available.

Starting a large machine in the above manner seriously disturbs the system, and its d. c. polarity when thus started is entirely a matter of chance.

Each of the stations feeds one or more of its lines in common with another station. By plugging the circuit-breakers at both ends of the lines thus extending from one station to another, feed for the lines of a station temporarily closed down may be obtained from neighboring stations. Current for starting is also thus provided. Three of the stations, including the one having the steam plant, have now also each a storage battery of 2,000 ampere hours capacity, which may, if necessary, be discharged in one hour.

Previous to the installation of all but the first one of these batteries, the starting of a station at times of heavy load was a serious matter, as the voltage at the d. c. bus-bar was generally very low, sometimes dropping to 350 volts, fluctuating with extreme suddenness through a range of 75 or even 100 volts. The rotaries, as first installed, received current, in starting, from the positive bus-bar, through the positive switch, series field, armature, starting rheostat, back to the negative bus; the negative switch being closed after the starting rheostat was fully cut out. Synchronizing was of course done by regulation of the shunt field. This, however, with the series field winding acting in opposition to the shunt winding, was, when the voltage fluctuated widely and rapidly, an exasperating task.

Heavy switches shunting the series winding were finally placed on the machines. Previous to that addition, however, the writer had the following method adopted by several of the attendants, and it proved eminently satisfactory, even more so than the use of shunting switches, inasmuch as the latter offer some resistance, especially at contacts, and therefore do not entirely eliminate the effects of the series winding which is itself of very low resistance.

In case all machines in the station were shut down, and No. 1 machine was to be started first, the following course was pursued:

The equalizing switches (located on the machines) of both No. 1 and No. 2 machines were closed; this connected the positive brushes of one machine directly to the positive brushes of the other machine No. 2 machine for the time stands idle, its series field circuit, positive cable and switch, together with the equal-

izer cable, serving merely the purpose of making connection from the positive bus-bar to the positive brushes of No. 1 (the machine to be started) at the same time leaving the series fields of No. 1 open circuited, and thus entirely inoperative. No. 2 positive switch is now closed, No. 1 positive switch being left open.

No. 1 machine is then started, by the usual means of the rheostat, as a simple shunt machine, and synchronizes beautifully under the most extreme conditions.

Variations of 50 volts produce extremely small variations in the speed of one of these machines when running as a d. c. shunt motor, while the variations in speed may be so great under the same conditions with the *old* method of starting, that only quick regulation and a cool head could prevent danger.

The positive switch of No. 1 is *not* closed until after the machine has been thrown on to the alternating line. At the moment of throwing power on the machine its connection with the d. c. line is *not* opened, as was necessary under the old method to prevent its loading up to the extent of blowing open the d. c. breakers.

As a *shunt* machine, it will pick up a load of 25 to 30 amperes. The fields are now regulated to reduce this load to about five amperes, and the positive switch then thrown in, when further regulation is made as desired.

To start one machine when the other is running, simply close both equalizer switches, leaving open the positive switch of the machine being started until after the machine is thrown into service. The process is otherwise the same as above described.

The above process absolutely avoids danger and is eminently satisfactory. The use of "shunting switches" is also said to give good results. In throwing a. c. power on a machine it is preferable to have the machine running slightly *faster* than it will be driven by the line, instead of *slower*, as a heavy armature in *dropping* into step will tend to feed the line, whereas the forced *acceleration* of an armature is apt to disturb the system to a greater extent.

The desired condition is, of course, easily obtained by increasing the speed of the machine while running as a d. c. motor, until it unmistakably passes the point of slowest fluctuation of the synchronizing lamps and said fluctuation again becomes more rapid. The speed is then slowly and continuously reduced to a satisfactory point where the fluctuations are still slightly above the slowest point, and the switches then thrown.

Any statements made in these notes concerning the operation of rotaries under abnormal conditions should not be construed as prejudicial to the use of synchronous rotary transformers as compared with motor generators, as, everything considered, the transformer is preferred.

The condemnation should rather be against the extreme con-

ditions which arose from the improper handling of the generators at the Falls, which would seem entirely inexcusable.

PRESIDENT STEINMETZ:—If there is no further discussion on this paper we will proceed to the next topic on the program for to-day, which is the reading of Mr. Wurts' paper on the Nernst Lamp. This paper has been printed in the TRANSACTIONS and has been distributed several times in the last few days, and I am sure is in your hands.

Discussion.

[See p. 511, June and July issue.]

[In connection with his paper, Mr. Wurts exhibited and described a sample of the six-glower lamp, as typical of all the sizes made.]

Mr. Blodgett:—What would happen if you used the glower for the three-glower lamp in a six-glower lamp, or vice-versa? You said that they were not to be used except in the lamps for which they were marked.

Mr. Wurts:—A glower marked for six-glower lamp will run at a lower efficiency in a single glower lamp, the difference being due to the higher temperature and consequently lower resistance of a number of glowers running together as in the six-glower lamp.

Mr. Hering:—I would like to ask Mr. Wurts what happens to the heater when the glower breaks or gives out in a single glower lamp. It seems to me the solenoid will then drop its little arm, and that will put the heater into circuit; the current then flows continuously through the heater. Is any provision made for this case?

If in the six-glower lamp several of the glowers have burned out, is the magnet still strong enough to keep the others in circuit? I noticed that the currents for the six glowers pass in multiple around the magnet, and when several glowers break, the force of the magnet will evidently be weakened.

I also do not believe Mr. Wurts stated what proportion the ballast resistance was of the whole resistance in order to produce that excellent characteristic that has been shown. It would be interesting to know what the proportion is.

Mr. Wurts:—In the single glower lamp there is no provision for keeping the heater out of circuit when the glower burns out. The heater, however, has a comparatively long life, and it is assumed that in general a single glower lamp will be turned off when it is found that the glower is burned out. In many cases, of course, the lamp will not be turned off for several hours, but, owing to the long life of the heater, this is not thought to be a serious matter. In the multiple glower lamp the heater is kept out of circuit by the current passing through two glowers; thus, an ample factor of safety is provided against cutting in the heater except for the purpose of lighting the lamp.

When the multiple glower lamp is turned on, the heater is cut out as soon as two glowers have lighted, the remaining glowers then lighting from the first two. What was the other question?

Mr. Hering:—The other, I understand, is answered in the paper. I did not see it. It would be interesting to have Mr. Wurts tell us something about the nature of the conductivity of these glowers. As I understand it, the Nernst filaments have an electrolytic conductivity, but it seems that it cannot be entirely electrolytic, from the experiments published. Does he consider

the conductivity to be partly electrolytic or entirely metallic? Is there any indication in the way in which the filaments finally break?

MR. WURTS:—I have not made a careful study of the electrolytic properties of the glower; the glower is generally understood to be an electrolyte. This is indicated by the action of direct current, and electrolytic action is absent on the alternating current. When a glower is about to break we find a rapid rise of potential, due probably to a crack in the glower, which soon breaks, forming a small arc for a moment until the circuit is ruptured.

Glowers that have been operated for six or eight hundred hours show a crystalline structure, and finally go to pieces. The exact process of disintegration is not clearly understood.

MR. J. W. HOWELL:—I think Mr. Wurts and his associates are very much to be congratulated on the work they have done. Each of the elements which enters into the construction of this lamp involves very considerable difficulties—the glower itself, with its connections, the heater and the ballast, and the three combined in one, constitute a piece of work the solution of which is very admirable. I wish to congratulate Mr. Wurts on it.

The characteristics of the glower, as shown in Fig. 1, are exceedingly interesting, and I do not think Mr. Wurts told us enough about them. If you study these curves you will see that any increase of voltage above the voltage of the top of that curve, which is about 197, will destroy the glower. Is that not right, Mr. Wurts?

MR. WURTS:—Yes.

MR. HOWELL:—So that the glower is designed to operate at a voltage, any slight increase of which will destroy it; this destruction is prevented by the ballast, and the very admirable way in which the ballast does it, as is shown on the curve a few pages subsequently, is a very pretty piece of work. In fact, the curve shows, that with an increase of voltage on the circuit there is actually a decrease of voltage on the glower. The lower curve, concerning which Mr. Wurts did not say anything, is to me very interesting—the action of the glower in vacuum. From that, it appears that taking the little circle which marks the point 1.76 watts per candle, that it occurs at a greater current, a lower voltage and a lower candle power. Now, if it occurs at a lower candle power it seems to me that it occurs at a lower temperature and that the glower itself will give better results in vacuum than it will in the air. If operated at a lower temperature its life ought to be longer, so that this feature, taken by itself, would indicate that you should burn it in a vacuum if it were not for the extraordinary amount of ballast which would be required on account of the shape of the curve. Now, am I wrong, Mr. Wurts, as to the temperature of the filament at the same efficiency in a vacuum? The candle power in air is 56; in

vacuum, 46, for the same glower. Does not the decrease of 10 candles indicate a decrease in temperature with candle power?

MR. WURTS:—Yes, I should say that it did.

MR. HOWELL:—Well, if it is a decrease in temperature, then the filament ought to last longer in vacuum than in air, at the same efficiency.

I would like to ask Mr. Wurts if the blacking effect we see occurs in a vacuum.

MR. WURTS:—As I stated in my paper, we have not continued these tests further than to determine that there was nothing commercial in them. The question as to how the glowers would behave in a vacuum or in gases presented itself and it was thought desirable to exploit in these fields; but so soon as it was learned that commercial advantages were not to be found in this direction our attention was directed along other channels. As explained in the paper, the commercial efficiency was found to be low.

MR. HOWELL:—The ballast made it prohibitive?

MR. WURTS:—Not only ballast but the heater necessary to light the glower; also how would you propose pumping out the globe? We would have almost insurmountable difficulties. We succeeded, nevertheless, in making up a few lamps with a vacuum and with automatic heater, but it was rather a difficult thing to do, and the results which we obtained indicated that the commercial efficiency of the device would be small.

MR. HOWELL:—On page 519 there is a statement that Dr. Nernst brought with him from Europe some glowers which could be started by an alcohol lamp, or even with a match, but that the glower required in running these lamps takes 950 degrees centigrade to start it. I would like to ask Mr. Wurts what is the relation between the temperature at which the glower becomes a conductor and the temperature at which it fuses,—or the highest temperature in which it could operate. Those which could be ignited by the striking of a match must ignite at a lower temperature than 950 centigrade, I imagine.

MR. WURTS:—Well, the glower will light at a lower temperature than 950 degrees.

MR. HOWELL:—Well, it seems that some light at lower temperatures than others and become conductors at lower temperature than others. Now, is there any relation between the temperature at which they become conductors and the temperature at which they fuse?

MR. WURTS:—There isn't any that I have discovered yet. I don't know that I have particularly investigated that point.

MR. HOWELL:—When you take a piece of ordinary porcelain it will fuse at a comparatively low temperature compared with magnesium oxide, for instance. Isn't that right?

MR. WURTS:—Yes.

MR. HOWELL:—And porcelain can be made a conductor at a very much lower temperature than magnesium oxide?

MR. WURTS:—Yes.

MR. HOWELL:—Now, is there any definite relation between the temperature at which they will become conductors and the temperature at which they will fuse?

MR. WURTS:—Well, there is a very decided relation there, but what it is or what kind of a curve it will produce, I do not know. Such material as glass, for instance, lights at a very low temperature, and if you take magnesium, or some substance like that, it will require a very high temperature, perhaps some substances will require an oxy-hydrogen flame to light them.

MR. HOWELL:—Those which require a very high temperature, such as the oxy-hydrogen flame, can't they be operated at a very much higher temperature than these substances which you are using?

MR. WURTS:—Yes, but they are very unstable. They will give you a horrible curve, and you have to put in such an amount of ballast that you can't get the commercial efficiency. Their point of efficiency is on that slope where you have the decreasing voltage with increasing current, and away down on that slope.

MR. HOWELL:—Then you cannot use in your lamps the substances which can be operated at the highest temperature?

MR. WURTS:—Why, they could be used.

MR. HOWELL:—I mean they cannot commercially?

MR. WURTS:—They cannot commercially.

MR HOWELL:—You are limited by the amount of ballast and by the temperature at starting?

MR. WURTS:—Yes.

MR. HOWELL:—About how much does the platinum cost in one of those six-glower lamps approximately?

MR. WURTS:—The platinum in the whole lamp?

MR. HOWELL:—The six-glower lamp.

MR. WURTS:—In the neighborhood of two cents, the platinum on each glower; and about 80 cents in the heater.

MR. HOWELL:—In the whole heater?

MR. WURTS:—Yes.

MR. HOWELL:—Ninety-two cents, about.

MR. WURTS:—Yes.

MR. HOWELL:—The development of the Nernst lamp abroad seems to have followed slightly different lines from what it has here. I have a letter from a friend of mine who has returned from Europe recently, and who states that in Berlin they are using Nernst lamps which give 25 candle power in a downward direction and 18 at the side, which consume 40 watts, and that there are 50,000 of them in use in Berlin, and yet I understand that your lowest candle power lamp is about 50. He also states that those lamps sell for a dollar, and are replaced for 25 cents. I just give you this as a piece of information.

I would like to ask on what data Mr. Wurts bases the statement, on page 525, that the Nernst lamp has a watt consumption a little less than half the incandescent lamp, for equal illumination.

MR. WURTS:—I base that statement on some measurements that were made with the Nernst lamp and incandescent lamps. That is, lighting measurements, floor and wall illumination. These measurements had been made by an incandescent lamp man. I took his result, and that was the general conclusion reached by those present.

MR. HOWELL:—I do not think, Mr. Wurts, that the table you show above bears that statement out.

MR. WURTS:—The table refers, you know, to arc lamps.

MR. HOWELL:—Well, the six-glower lamp—you gave the figures there for six-glower lamps, and I can give you all you want on incandescent lamps. The illumination of two lamps can only be compared by the spherical illumination, or else by their illumination under exactly similar conditions. If you want to compare the downward illumination of two lamps you must place them in the same conditions, with reflectors placed above; but if you eliminate the reflectors, you cannot compare the light of the lamp in any direction, and the spherical candle power is the only correct comparison that you can make. In the table here the spherical illumination of the six-glower lamp is about 3½ watts per candle, and that is not American candles, either. It is European candles.

MR. WURTS:—They are all reduced to the same.

MR. HOWELL:—Well, there are no incandescent figures here, though. I am talking about the incandescent lamp. In order to compare the Nernst lamp with the incandescent lamp, there is one element which is absolutely necessary, and that is the life and candle power curve in the lamp. If you base the statement on initial ratings only, it is impossible to form any comparison, because you can make the incandescent lamp efficiency anything you like, down to ½ watt per candle. So that the bare statement of efficiencies is of no value whatever in comparing two lamps. Now, there is no statement in the paper as to the life and candle power curve of these lamps, but if you take the initial spherical candle power as 3½ Hefner units, the ordinary 3.1 watt incandescent lamp has a spherical efficiency of 3.28 Hefner units, which is a lower figure than the other, and the ordinary 3½ watt lamp has a spherical efficiency during its useful life, which is about 800 hours, of 4 watts per spherical candle—

MR. WURTS:—Three and a half, 800 hours.

MR. HOWELL:—Hefner units; that is, not American units. Now, I think the comparison made by Mr. Wurts is not a fair one, as to the spherical candle-power, due to the fact that there is no life or candle-power curve of the Nernst lamp given. I

think it ought to be modified. In fact, if this glower indicates the condition in which it is after 100 hours' use, the blackening would indicate a considerable loss in candle power, and if it needs cleaning in 100 hours it would have to be taken out of its case and practically a new one put in. Of course, the expense would not be very great, but the useful life of the operative lamp is the time before it has to be renewed; and I think this lamp is ready for renewal now.

I would like to ask Mr. Wurts how a lamp on direct current breaks? He says it lasts only half as long as the regular one.

MR. WURTS:—It usually breaks at the terminal, where there is electrolytic action, and where this platinum black deposits in the pores. In regard to the question just before the last one, as to the statement on page 531, I cannot say that I have exactly reproduced the conditions of lowering voltage and having the voltage come on again. But we have frequently had the lamps go out; lamps that have been burning on commercial circuits, and had the current cease and immediately come on again as they were switched over in changing the dynamo. And we haven't yet had any instance in which fuses were blown or any trouble occurred.

You will notice in the curve on page 529 that the current which is on the heater, at 3¼ amperes, is on the circuit for only a moment, and if the current is turned off the lamps and turned on again within, we will say, four seconds, the glowers will immediately light again.

MR. HOWELL:—It will hold its heat four seconds?

MR. WURTS:—Yes; and if the current is turned on again at the end of four seconds, the cut-out will instantly open and the glowers will again light from their own heat, and the action of the lamp under such circumstances would not affect the power house.

MR. HOWELL:—That would not; but if you had to wait thirty seconds it might.

MR. WURTS:—If you had to wait thirty seconds, the glowers would become practically cold and the lamp would simply light up from normal again, according to this curve.

MR. HOWELL:—But the heaters take a higher current.

MR. WURTS:—They take the current shown in this curve (indicating).

MR. HOWELL:—That isn't very much.

MR. WURTS:—For an instant of time the current taken by the heater when cold, is slightly in excess of the normal current of the lamp, but this is so quickly reduced, owing to the positive correction in the heater, as to make it a matter of no consequence whatever. We have never experienced any difficulty whatever from this source.

With reference to the lamps made in Germany, we have not yet made any attempt at smaller candle powers than 50. We

purpose in the near future to have a 30 c. p. lamp, but in our judgment, anything smaller than this would involve such delicate apparatus in the interior of the lamp as to make it unfit for ordinary service. I may be mistaken, but so far, we have not thought it worth while to attempt anything lower than 30 c. p. The small lamps referred to, which are made abroad, have a comparatively small radiating surface and in a number of these which I have tested, I have found the temperature of the lamp sufficient to destroy, in a comparatively short time, some of the materials in the interior; for example, I found that the cut-out coils were provided with silk insulation and that the ends of the coil were made of fiber. After running about ten hours, these combustible materials were found to be black, entirely burned out, rendering the lamp useless for further service. There were other features in these small lamps which did not appeal to me as practical and which made me feel that these lamps were not commercial.

With regard to efficiency, I have begun my section on this topic by saying that it is not an easy matter to state the efficiency of the light, to the satisfaction of all concerned. I knew very well when I wrote this paper that these questions would come up and I do not think that they are settled yet by any means. "The proof of the pudding is in the eating," and we know from the comparisons that have been made that we have in the Nernst lamp a lamp which, for the same consumption of power, will give a better illumination and a superior quality of light, no matter what the measurements on the photometer may indicate. We know these facts to be absolutely correct, and these facts are what the customer wants to know; he wants an effective and efficient light. We have designed our six-glower lamp—a large unit—so as to throw all the light downward where light is ordinarily wanted, in factories, dry goods stores, railway yards, and places of that character. In such places, all the light is required on the ground, on the floor, or on the counter and not on the walls. Of course if it is desired to diffuse the light, this may be effectively done by the use of globes.

PRESIDENT STEINMETZ:—Gentlemen, I must say, before I give the floor to the next speaker, that in discussing the merits of an apparatus newly introduced, which is here brought to our notice for the first time, and which has been for years an entirely experimental apparatus and is now just being changed over into commercial form, at least so we hope, it is not fair to compare it with an apparatus established by experience, and successfully established, for many years, such as the incandescent lamp. We must expect in such a novel and recently developed apparatus that there will be a number of features which under certain circumstances may be objectionable, and which must be left for their development to practical experience in the use of the apparatus in commercial operation; as, for instance, this relatively

large starting current, which in many cases would not be objectionable, but in other cases would be objectionable, as in the lighting of a whole system fitted with these lamps. As soon as found to be objectionable, I have no doubt whatever that means will be found to eliminate the difficulty, and so as to all other features which are perhaps not quite satisfactory yet under certain circumstances, and which our experience merely has to make satisfactory. So it is not quite fair to compare the Nernst lamp, which is an entirely new apparatus, just beginning to be introduced commercially, with the well-established incandescent lamp.

Regarding these candle powers, I may say that a consumption of 3½ watts per candle for the Nernst lamp appears to be abnormally high, but if you go to the next column you get consumptions for the arc lamps of over three watts per candle. Now, that is very much higher than we are accustomed to consider arc light consumption, and I am led to believe that in measuring the efficiencies certain conditions existed different from those usually chosen, and that therefore these efficiencies do not compare directly with what we call efficiencies in incandescent lamps, of three watts per candle. Such an assumption would support the statement of Mr. Wurts, that the Nernst lamp has a much higher efficiency than the incandescent lamp, but if the above figures are taken, the Nernst lamp would come below the high economy incandescent lamp.

I would like to ask Mr. Wurts, with regard to this black deposit which forms on the glower in operation with direct current, whether he has determined the nature of it, whether it is platinum or whether it is electrolytic metal, and furthermore, whether he knows what is the approximate temperature of the filament or glower, in operation?

MR. WURTS:—I cannot say that I definitely know the nature of the black deposit on the glower. I think, however, it is platinum black. As to the temperature of the glower, I have really forgotten. We made some rough determination of about 2500° centigrade. It is rather difficult to get these temperatures accurately, but it was something in that neighborhood.

PRESIDENT STEINMETZ:—Was it determined by estimation from the light radiation or by calorimetric tests?

MR. WURTS:—Calorimetric tests. With reference to photometric measurements on the lamp, I would like to call your attention to the quality of the light. Possibly this would make some difference in a comparison made between Nernst and incandescent lamps. If you will look at the Nernst lamps in the Electricity Building and then look either way up or down through the main aisles, you will notice the light giving power of the Nernst lamps. What a glorious light it is that is thrown down on the floor! You will be impressed with the volume of light that seems to be emitted from each lamp. Then look at the

light given by the arc lamps. They are bright spots to look at but they fail to illuminate the floor.

With regard to the blackening of the holder which Mr. Howell spoke about, it does not seem to me that this is anything like as serious as he would make it out. It is nothing like as serious in my judgment, as the blackening of the bulb of the incandescent lamp, for in one case you can clean it and in the other case you cannot. In a large majority of cases, incandescent lamps are run to death simply because the lamps continue to burn and give out some light, but they are then burning at a very poor efficiency, whereas with the Nernst lamp, a few strokes of a scraper will remove the black deposit on the holder and the efficiency is at once thereby renewed. Where these lamps are used in any quantity, it seems to me that the cleaning and care of them is a very insignificant matter.

Mr. W. J. Hammer:—Mr. President, I would like to supplement Mr. Howell's endorsement of the beautiful and efficient work that has been done by the Westinghouse engineers in this country in connection with the development of the Nernst lamp. I had the pleasure during the year past of spending more than five months in Europe and visiting practically all the places where work is going on with the Nernst lamp and seeing practically all the types that are being made in Europe. I also saw the American types of lamps abroad and in this country, and I was exceedingly impressed by the character of the work which was being done by the American engineers. The Nernst lamp is an illustration of that which we have seen before in the case of foreign inventions that have come to this country which have been made far more commercial and shipped back again to the foreigners. There are several points in the present type of Westinghouse-Nernst lamp that particularly appeal to me. One is Mr. Hanks's very interesting clamp, and the other the getting rid of the spiral heater as it is used abroad and the substitution of a very much improved form. The little wrinkle of the recess or indentation around the openings where the leading-in wires pass, so as to prevent the settling of platinum black around the connection, is a very good thing.

There are one or two things I would like to ask Mr. Wurts. I know at the time I was in Berlin there seemed to be considerable dissatisfaction with Mr. Potter's very beautiful iron steadying wire, or ballast, and I understood that there was considerable being done with platinum and special resistance wires as a form of ballast. I would like to know to what extent the iron steadying resistance or ballast which is here employed has been displaced by platinum or special resistance wire. I noted also in England that they had considerable difficulty due to the expansion of the glower body itself. I saw certain types of lamps, and have some of them in my possession, in which, instead of the glower being held rigidly, one end was fastened to a tiny platinum wire, which

passed through a platinum wire loop or spiral of the supporting wire so that it could have a side play, and the end of the tiny wire attached to the glower was brought down and attached to the supporting wire below this loop, and I was informed that this had gotten rid of a very considerable percentage of breakages due to expansion of the glower. I note that this trouble is very nicely obviated in the present type of lamp, but I think it would be interesting for us to know to what extent that difficulty has been met with here. The Welsbach mantle is so closely related in certain characteristics to the Nernst lamp that there is another very interesting point I should like to bring up. I believe the Welsbach mantle falls off in its efficiency about 25 per cent. in the first 60 hours of burning. I would like to know if Mr. Wurts can tell me about what the falling off in efficiency would be in the standard Nernst glower, say in the first 100 hours. That is, of course, leaving out of consideration the use of any powdered substance which can be removed with the platinum black upon it. I merely wish to know what the depreciation is in the glower itself.

There have been a good many strong statements made as to what the Nernst lamp is going to do to the arc and incandescent lighting industries. You know what Mr. Swinburne and others have said upon the subject, as to the ultimate effect of the Nernst lamp upon the incandescent and the arc lamp, and I think it would be very interesting to us all if we could get an expression from Mr. Wurts as to just what relative position the Nernst lamp will hold as compared with the incandescent and the arc lamp.

Mr. Wurts makes the statement in his paper that the alternating current lamp has a life of about 800 hours. He does not say what the life of the direct current lamp would be, and I think it would be interesting if he would state about what the results of their tests are. Of course, that feature—the life of the lamp—probably would improve as time went on. As our president says, this is a brand new thing, but at the same time it is a very hearty, robust infant, and we would like to know, in the present condition of affairs, what the life is of a Nernst lamp on direct current, whether it exceeds, say, 300 hours.

There was a reference in Mr. Wurts's paper to the use of platinum as a heater, and its advantages, and in reference to its cost, and in that connection a gentleman in attendance upon this meeting (I do not know whether he is here in the audience or not) showed me a few days ago some samples of a metal which he claimed had all the characteristics of platinum, and that a substance had finally been found which would displace platinum. I tried to get some further particulars, but did not. That gentleman may be present in the audience and may be willing to say something on the subject. But he did say that while it could be made for much less than the cost of platinum it would probably

be put on the market at perhaps 25 per cent. less than the cost of platinum. This was not given to me in confidence, and if the gentleman is present this morning I think it would be very interesting to hear from him on this subject.

In going about on the other side of the water I noticed a number of places where the torch lighting form of lamp had been replaced by the automatic type, and it is self-evident that the automatic type of lamp is the lamp that is best suited and best qualified to fill the bill. I would like to ask Mr. Wurts whether it is proposed by his company to put out any lamps of the torch lighting type or not?

Mr. Howell made some reference to the cost abroad. I think the cost is a little bit in excess of what he stated, but the rental, I think, is about 72 cents a year. I do not know whether the company will adopt the policy of outright sale entirely or whether they will do as the foreigners do, install the lamps at an annual rental of 72 to 75 cents.

One other point—I think Mr. Howell referred to it—in reference to the use of the Hefner standard, which had been used in this paper, it is stated, so as to enable a more ready comparison to be made with the foreign practice and with the public statements regarding the Nernst lamp. It would seem that a statement showing a comparison with the standards of light used in this country would perhaps be of equal if not greater interest to those who are interested in the application of the incandescent lamp.

PRESIDENT STEINMETZ:—Gentlemen, the time is getting rather late, so I must ask you to be brief as possible. At the same time the discussion is very interesting, and we do not want to curtail it, though we desire to have it as short as possible.

MR. WURTS:—In regard to the falling off of candle power in the glower irrespective of the discoloration of the globes and porcelain parts of the lamps, I would say the loss is slight, possibly 3% in 100 hours. This loss is small compared with the loss of candle power in the lamp when it is neglected.

With regard to the shrinkage of the glower we have provided against any trouble from that source by making the glower and the leads U-shaped so that if there is any expansion or contraction it can take place without any strain. Soldered connections are used abroad, and for that reason the glowers and heaters are not easily or quickly replaced. In the American lamp we have taken particular pains to make the perishable parts easily and quickly replaceable.

With regard to the life of the direct current glower, I may say that we have done very little work in this direction for the following reasons: When we started with our work on the Nernst lamp we were aware that our friends on the other side were working with direct current only, and that they were working with small candle powers and it did not seem worth while to

parallel their work. We started out on lines of our own. We decided to attack the problem from the other extreme, to make large candle powers and to use alternating current. We have made a few direct current lamps. They are now in operation, but we have not succeeded in getting more than 300 hours out of the glowers. I am of the opinion, however, that we shall eventually get a longer life than this from the direct current glower. We have tests under way which indicate this, and we are hoping to get 500 hours but I do not look for anything much better than this.

If the gentleman Mr. Hammer spoke of is present, I would be pleased to hear from him regarding that metal. If he is not here I may say a word or two.—I have tested the metal referred to and have found that it is not serviceable for the purpose of our heater; the temperature in the lamp is too high. The results did not compare favorably with the results obtained from platinum. So far platinum wire makes the best all around heater.

With regard to a torch lamp, or match lamp, I would say that the Nernst lamp in this country is the personal property of Mr. Westinghouse; it is not owned or controlled in any way by the Westinghouse Electric & Manufacturing Co. I am not yet informed as to what Mr. Westinghouse's plans may be with regard to the company which he will no doubt form. He has already prepared a large factory for the manufacture of these lamps in Pittsburg, and we purpose moving into that factory in about two weeks. We shall prepare to turn out these lamps in large quantities and hope soon to be able to fill the large orders which have already been received from all parts of the country. I doubt very much whether Mr. Westinghouse will ever make a torch lamp; it would not be popular in this country. We want a good simple lamp which can be turned on and which can be substituted for an incandescent lamp.

Mr. Hammer, you asked some other questions with reference to arc and incandescent lamps.

Mr. Hammer.—As to the relative position, in your estimation, to be occupied by the Nernst lamp. You will remember that Mr. Swinburne said as to what the Nernst lamp would do to the incandescent industry, in his Society of Arts lecture of February 8th, 1899: "Once the Nernst lamp becomes so general that systems of distribution are laid out to suit it instead of the carbon lamp, the carbon lamp is practically 'out of the running'"; and Dr. Blathy told me in Budapesth last October, that when the Nernst lamp became a commercial article there would be no more arc lighting. Now, with your experience I thought it would be interesting to know what the relative position of this lamp would be, as compared with the arc and incandescent lamp.

Mr. Wurts:—In my judgment, the Nernst lamp will be a sharp competitor of the arc lamp; not so much of the incan-

descent. I think it will be largely used in dry-goods stores. One party said that the Nernst lamp would save them many thousand dollars a year owing to the fact that with incandescent illumination, ladies were unable to match materials, or if they attempted to do so, the purchase would be returned the next morning as unsatisfactory. I wish you would all examine the arrangement we have in the Electricity Building for showing this beautiful daylight quality of the Nernst light. Colored silks appear in their true shades and are matched as easily as by daylight. I think that the Nernst lamp will be largely used in factories, railway stations, railway yards and the like. One of the officers of the Pennsylvania Railroad Company the other day said: "You have no idea of the trouble we have in the yards from the shadows and flickering of the arc lamps. Locomotive drivers have difficulty in distinguishing switches and signals," but, he said, "that lamp is just what we want in the yards, a perfectly steady and shadowless light." I think that the beauty of this light as it appears in the Electricity Building will appeal strongly to all of you, and it is likely to be a competitor of any other illuminant where quality of light is a consideration. The fact that the single glower lamp requires twenty-five seconds to light, may prevent its being such a sharp competitor of the incandescent lamp as might otherwise be. Nevertheless, where incandescent lamps are grouped under a shade, I think the Nernst lamps will be found more desirable, more efficient, and lend themselves better to decorative illumination. For machine shop purposes, the absence of the shadow and flicker which accompanies the arc lamp, has made the six-glower type of Nernst lamp popular wherever it has been installed.

MR. OBERLIN SMITH:—How would it be for railway car lighting?

MR. WURTS:—You mean for street car service?

MR. SMITH:—No; steam cars, where electricity is used.

MR. WURTS:—It would make a very pretty light there. I don't know why they should not be used there as well as incandescent lamps. They would have the same advantages there as in a parlor or sitting-room.

MR. STILLWELL:—I should like to ask Mr. Wurts whether he has made any tests, such as he has spoken of, of the Nernst lamp on 25-cycle current. His test as I understand was made on 60 cycles. It occurs to me that if the Nernst lamp is entirely successful on 25 cycles, and I do not see any reason why it should not be, it has rather an interesting bearing on the general power transmission plants in the country. The only argument in favor of 60 cycles that has been given any very great weight recently, is that it enables you to operate arc lamps on the same circuits as are used for motors and incandescent lamps. If the Nernst lamp is applicable to 25 cycles it would be of very great value on these extremely long transmission systems that Dr. Perrine is interested in, in California, where a high frequency is objection-

able for many reasons in connection with transmission, but yet where the desire to use arc lamps sometimes leads to the use of a frequency undesirably high.

MR. WURTS:—The lamp is operative on 25 cycles. As far as our tests with glowers go, we have not discovered any difference between 25 and 60 cycles. We cannot notice any flickering. The only difficulty we have encountered has been in the cut-out. The lamp before you was designed for 60 cycles, but when placed on 25 cycles—well, the cut-out does not hum exactly, it rattles. Thus we have a new problem to solve. We have now succeeded in designing a cut out which will operate on either number of cycles without noise or hum. You may hold your ear immediately beneath the dome shade and not hear a sound.

MR. HERING:—What is the frequency of your current here at the Exhibition?

MR. WURTS:—Sixty cycles.

MR. STERN:—I would like to ask Mr. Wurts one or two questions regarding the strength of the glower, when the lamp is under heat, as it might be effected by vibration. The question might come up in car-lighting, or in contemplating the use of these lamps for automobiles where considerable vibration is encountered in running over rough roads, and it occurred to me that while in a heated condition, the vibration might cause the rupture of a glower, thus shortening the lamp's life.

Another question is one as to its actinic power when used in photography. A lamp of this kind might be of considerable use in photographic printing, copying and the like, as a better means for getting a steadier light than the arc lamp, were its actinic power sufficiently high to permit of short exposures.

PRESIDENT STEINMETZ:—Are there any more questions to ask?

MR. ALEXANDER:—I would like to ask Mr. Wurts a question or two. As I understand it, the heaters of the lamps are cut out when two glowers are burning.

MR. WURTS:—In the six-glower lamp.

MR. ALEXANDER:—If that is the case, is it necessary to have four heaters? Would it not cheapen the lamp, lessen the blackening and cut down the high starting current of the lamp if less than four heaters were used? Inasmuch as two-glowers bring the other glowers up to conducivity, why would it not be necessary to have only sufficient heaters to bring up to conductivity two-glowers, and let them set the other glowers active?

MR. WURTS:—With regard to the strength of the glower when hot, I would say that the glower is by no means brittle at that temperature. If a glower is too long and heavy, it will sag, but if properly proportioned, it will remain almost perfectly straight. I do not think there is any danger of glowers breaking in car service, although I do not speak from experience.

I am not able to give any information regarding the photographic properties of the Nernst light. As yet, we have not

made any investigations in this direction. I can imagine that the light might be useful in this sphere.

The reason we use four heater tubes to light two-glowers is this: Suppose that the two-glowers immediately underneath the two heater tubes were burned out, then the other glowers would not light. We must be able to start any two of the six-glowers and this requires four heater tubes. The principal idea, of course, in cutting out the heater when two-glowers are lighted is to save the heater from being cut in when three or four-glowers are burned out. With only two heater tubes, there would certainly be danger at times of not lighting the lamp.

MR. HOWELL:—I got a wrong impression from page 531, Mr. President, where I spoke of the normal current of a lamp as $\frac{3}{4}$ ampere, and starting current $3\frac{1}{4}$. That is a mistake. The normal current is not $\frac{3}{4}$, but 2.3 amperes. That .75 was for one glower, so that the short-circuit effect I spoke of would be nothing.

PRESIDENT STEINMETZ:—Gentlemen, we have had a very interesting paper and a very interesting discussion. I may draw your attention to one feature. If there are any further questions to ask they might be asked by communication and will be submitted to Mr. Wurts, and he can answer them by communication, so that if any question has been left unasked to-day you still have a chance to ask it and to get an answer.

[Adjourned to Saturday, Aug. 24.]

[COMMUNICATED AFTER ADJOURNMENT.]

PROF. A. J. ROWLAND:—As I listened to the discussion of Mr. Wurts' paper, I was impressed by the importance of the number of candles power given by a light source. In his paper, Mr. Wurts adheres pretty carefully to the lamps as being 1 glower, or 6 glower, and the like, only mentioning candle power when the efficiency of the light is considered. In the discussion, however, and at the Westinghouse Company's magnificent display in the Electricity Building, constant reference is made to their candle power, while commercially the lamps are rated as 50 c. p. and multiples of that (p. 521 of the paper). This figure is evidently the "end on" candle power. In the Nernst lamp, then, we have the one first to be *commercially* rated for candle power and efficiency upon the "end on" candle power, the importance of which is recognized by Mr. Wurts in his paper by designating it as desk illuminating power. Personally, I not only think this of the highest importance, but one of the things which will make Nernst lamps of the utmost value. I shall be greatly surprised if it does not have a most important bearing on the commercial rating of other artificial illuminants, especially those using the electric current, when the Nernst lamp comes into competition

with them. This is not an argument for "end on" candle power as the important one; but a question, why in these lamps, which must compete with lamps of other types in present use, the glowers were not placed to give maximum candle power from the sides of the lamp, naturally suggests itself.

As I have thought of the Nernst lamp and its probable future, now that in the hands of Mr. Wurts and his assistants it has been prepared in such complete and careful way for American service, it has seemed to me that it must take a place which is filled neither by arc nor incandescent lamps. In fact, the Nernst lamp seems to be a sort of cross between the arc and the incandescent lamp, in which some of the faults of each are suppressed and the good features retained.

The incandescent lamp, as usually constructed, is very deficient in "end on" candle power; the arc lamp throws no light directly below it, except by diffusion through the globes. The Nernst, on the contrary, gives its maximum candle power in the "end on" direction, or directly below it. Even Mr. Howell, in his remarks, recognized the importance of desk illuminating power by noting the fact that the arc lamp fails by not sending its light below it.

The Nernst lamp somewhat suggests the enclosed arc lamp in the form in which Mr. Wurts has shown it, and has its merits, but it has others which the enclosed arc lamp lacks. The light is in the right place, the efficiency is high, the color of the light is very good, and the light is steady. I do not think that Mr. Wurts gives full credit to the real efficiency of the Nernst lamps as compared with arc lamps, for the latter have their candle power cut down so very much indeed by globes (put on for the sake of producing better light distribution or to suppress the purple color of the arc itself) that a *maximum* candle power of 200 from a 5-ampere 110-volt lamp is all that is procured.

The trimming of a Nernst lamp is not much different from the trimming of an arc lamp, but the ease with which the smaller sizes at least may be removed as one would remove an incandescent lamp, so that such repairs as would be impossible for an incandescent lamp may be done on them, is very noteworthy. By this method of construction the new lamps are adapted to interior work where lamps supplying light with poor color could only be used in the past.

I do not believe that the color of the light from the glower of a Nernst lamp can be considered equivalent to daylight, however. An ideal artificial light must have color like daylight as one of its requisites. So far as I am aware, no experiments have been made which show that the light from the Nernst lamps has this quality. Matching colors by incandescent lamps and then by Nernst lamps does not show it; even an analysis of the light into its spectrum determines little in this direction. The light from incandescent gas lamps (when a mantle of rare earths

is used) is often called cold, ghastly and the like; and there appears to be an absence of the red rays, too plentifully present in nearly all artificial lights. I hope this fault is not one to be found when rare earths are used as a light source by being heated with the electric current, but cannot see how it can fail to be so.

One of the speakers in the discussion has asked questions regarding the depreciation of candle power with life in the Nernst lamp. In this connection I want to say that I am very confident that the depreciation in candle power of Welsbach mantle lights (which give the best example of the use of rare earths in lighting work) is immensely overstated. I fear that most figures one sees have been prepared by those who have most to fear from the competition of improved gas lighting. I speak with a knowledge of the most elaborate experiments which have been carried out on Welsbach mantles in this country, when I say that—burned as Welsbach mantles commonly are, at the highest candle power attainable with the gas volume and pressure available from a given burner—the average depreciation is not over 10 per cent. for the first hundred hours, quite a bit of which is due to shrinkage of the mantle to the shape of the unseen flame within. The life of a mantle is more than comparable with the life of the ordinary incandescent lamp. I do not believe the Nernst lamp has to fear failure from rapid depreciation in candle power. By suitably regulating the temperature of the glower and its efficiency, doubtless results quite as good as with incandescent lamps can be obtained.

With regard to the possibility of the Nernst lamp operating on 3,000 alternations, attention may be called to the fact that all the incandescent lighting of the Pan American Exposition is with alternating current at 3,000 alternations. It takes a very close observer to see any flicker, in spite of the fact that the candle power of the lamps is low and the filaments therefore very slender, so that their heating and cooling would readily follow the fluctuation of current through them.

Discussion.

[Discussion on the papers of Messrs. Armstrong and Berg, Buffalo, Aug. 24, 1901.]

President Steinmetz:—Gentlemen, before proceeding to the discussion of these two papers, I will call upon Mons. Janet, who desires to say a few words to you.

Mons. Paul Janet:—Now that my technical remarks are ended, I must not forget that to-day is the day of parting, and that soon we shall leave each other for a long time. Some two weeks ago, in New York, I was expressing to you, in the name of my fellow-countrymen, all the sentiments that your warm welcome had inspired in us; since that time you have overwhelmed us with the most delicate attention; the day passed in New York, on the East River, the sail on the Hudson River, the charming reception at Schenectady; in a word, all this entire week passed in Buffalo, Niagara, at the Exposition, in the various stations of the city, all that will leave ineffable recollections with us. The technical visits that we have been able to make in company with you have been highly instructive to us; we have especially admired the magnitude and power of your installations, and, in each of them, the great care which has been devoted to the study of the equipment. This portion, which was formerly considered a mere accessory, assumes to-day the highest importance, and we have been struck with the considerable efforts that you have made to bring it to the same degree of perfection as the steam engine and the dynamo; the switches, the circuit-breakers, all the control and safety appliances, become real machines, really mechanical and robust in construction, and they take the important place which belongs to them.

It would take too long to enumerate here all the other objects of our remarks; this will form the material of a report that we will prepare on our return. For the moment, allow me to express to you, for the last time, the sentiments of profound gratefulness that we feel for our reception, and the hope that our trip will have contributed to secure, in a more intimate manner, the bonds of fraternity between the American Institute of Electrical Engineers and the Société Internationale des Electriciens.

President Steinmetz:—Gentlemen, I believe you will join with me when I assure Mons. Janet that the pleasure has been mutual, and while we are very glad to have been able to make him comfortable here and to entertain him, we have profited just as much by his presence and the presence of his friends, because there is nothing which broadens the views of men, especially of engineers, so much as intercourse among those who have studied the same problems from different points of view or in different countries; and beside this, the intercourse which now exists between ourselves and our friends from abroad, is a

means of strengthening the bond which ever has connected all the engineers, all the scientific men, from the various countries with one another and is the outward sign of the unity of interest and the unity of ideas and of sentiment existing between them.

Gentlemen, before proceeding with the next paper, I call upon the Secretary to read some resolutions which have been brought up by the Library Committee for your further action.

Secretary Pope read the following resolutions:

WHEELER RESOLUTIONS.

Whereas, Dr. Schuyler Skaats Wheeler, a life member of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, has purchased the extensive collection of electrical books and periodicals, made by the late Latimer Clark, and has presented it to the INSTITUTE, on certain conditions as to its future preservation and maintenance.; and,

Whereas, The Council of the INSTITUTE has accepted the gift on the terms stated by the donor; and,

Whereas, The library, which thus becomes the property of the INSTITUTE, comprising 2,048 bound volumes of books, 3,450 bound pamphlets and 1,378 volumes of periodicals, is the most complete collection of early electrical literature in the world. being especially rich in the rare publications of the seventeenth and eighteenth centuries, and containing practically all modern works up to 1886; now, therefore, be it

Resolved, That the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS hereby acknowledges to Dr. Wheeler its high appreciation of this magnificent gift, and of the generous spirit which he has shown in thus placing this valuable library at the service of his fellow-members, and at the same time laying such a substantial foundation for the future library of the INSTITUTE; and be it further

Resolved, That these resolutions be engrossed, and that one copy be presented to Dr. Wheeler, and another copy be suitably framed and hung in the Library of the INSTITUTE.

CARNEGIE RESOLUTIONS.

Whereas, The AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, through the generosity of one of its life members, has become the owner of the Latimer Clark electrical library, containing almost 7,000 titles of books, pamphlets and periodicals published prior to 1886; and,

Whereas, In order to make the library of the greatest use to all members of the INSTITUTE and to the electrical profession generally, a large sum of money was needed for the preparation of a printed catalogue and for the purchase of additional volumes; and,

Whereas, Mr. Andrew Carnegie has generously contributed $6,880 for these purposes; now, therefore, be it

Resolved, That the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS hereby expresses to Mr. Carnegie its high appreciation of his generous gift and of the interest which he has shown in the welfare of the INSTITUTE by thus assisting in so substantial a manner to establish and make accessible a library

which shall contain the entire literature of electrical science; and be it further

Resolved, That these resolutions be engrossed, and that one copy be presented to Mr. Carnegie, and that another copy be suitably framed and hung in the library of the INSTITUTE.

MR. MAILLOUX:—I take pleasure in moving the unanimous acceptance of these resolutions.

MR. C. A. BRAGG:—I second the motion, Mr. President.

[Carried unanimously.]

PRESIDENT STEINMETZ:—Gentlemen, discussion is now in order on the papers of Messrs. Armstrong and Berg. We have here with us several prominent engineers from foreign countries, who undoubtedly have given the problem of three-phase alternating current railroading a good deal of attention, and we should be very glad to hear from them. Mons. Gillon, will you not say a few words to us?

PROF. G. GILLON, (University of Louvain, Belgium): [Translation]—I regret very much not to have sufficient knowledge of English to be able to express myself in your language, but that would be all the more difficult, inasmuch as your President has called on me unexpectedly, in consequence of which I would be much embarrassed to tell you anything interesting. I do not possess any personal experience on the subject which is at present before us, but I have had occasion to follow, from quite near, some recent experiments which are still in process at Berlin. These experiments relate to electric railroad traction at very high speed (125 miles per hour). It is noteworthy that for a long distance line which requires locomotive capacity of 1,600 h. p. normal output, we could scarcely think of utilizing continuous currents at 500 volts, since the expense for copper and the number of sub-stations would both become entirely too great.

It is, therefore, natural to think of triphase currents at 10,000 or 12,000 volts, especially since the principal objection to the triphase motor, notably that of starting with difficulty and of absorbing a large amount of energy on grades as the result of its speed constancy, would disappear in certain cases, for example—that of a long line at very high speed on ground having few slopes and where only few stops are necessary. Under these conditions I see a future quite near and quite extensive for traction by means of triphase electric motors. It is this conviction which has led the firm of Siemens and Halske to make certain experiments, of which I will say a few words.

About two years ago, this firm put on trial on a line two miles long, a locomotive of 300 h. p. capacity, taking current at 10,000 volts from a triphase overhead line by means of three rubbing trolleys. The high tension current from the line passes through the transformers which furnish it to the motors at 700 volts. I have had occasion to see this locomotive in operation. Its

starting is rapid enough, and the trolleys make satisfactory contact with the line, although it should be stated that the speed was only about forty miles; in consequence of the short length of the line it was naturally not possible to attain higher speeds, but these experiments seem nevertheless to have proved the possibilities of a triphase line operating under 10,000 volts, notably from the standpoint of circuit insulation of the trolley connections and of the current and power control. It remains, however, to be established whether the speeds desired can be attained, and especially whether these speeds will be attained with an economically possible efficiency. It is in order to establish these points that the firm of Siemens and Halske has constructed a motor car of 90 tons, with 4 motors, having an aggregate capacity of 3,200 h.p. in starting, and 1,600 h. p. mean working output. This would constitute by itself a high speed train having seating capacity for 60 to 70 passengers. It must be, by this time, actually constructed, and will be tried shortly on a line of 28 miles between Berlin and Zossen. Some analogous experiments will likewise be made by the Allgemeine Electricitäts Gesellschaft on this line. It is to be hoped that these efforts will be crowned with success and that the electrical industry will see opening to it an avenue of usefulness as new as it is important.

PRESIDENT STEINMETZ:—Gentlemen, we have listened to Mons. Gillon's interesting remarks. Now we should like to hear from another country. Perhaps Dr. Janisch can enlighten us further, as he is connected with the Siemens company which has done quite a good deal of experimenting in railway matters.

DR. JANISCH:—I am, unfortunately, not in the position to give you material information in regard to the question under discussion, since I am not directly connected with that department of Siemens and Halske which is making comprehensive experiments on this subject. I know that my firm is running an electric locomotive with 10,000 volts three-phase current and maximum speed capacity of 140 miles per hour experimentally on an experimental track in Gross-Lichterfelde in the neighborhood of Berlin.

So far as known to me, these experiments are to be carried on in common with other electric companies of my country on the railway line between Berlin and Zossen. The extent to which the industrial utilization, on a large scale, of the experimental results obtained may become possible in the future, depends in reality on the encouragement of our authorities, who, unfortunately, are not always disposed to follow the rapidly advancing technology with intelligence, and do not always understand how to impart to a healthy development of our industry that propelling force which you here call "push," and which has made you so great in such a remarkably short time.

I beg you for to-day to be satisfied with the small amount of information which I am able to communicate to you, and beg to

be allowed, after my return to Germany, to communicate more complete information, perhaps, in your TRANSACTIONS, concerning the character and results of our experiments. I still have to thank your President most heartfully for the exceedingly great honor which your highly honored society has done me by singling me out to be its guest during this meeting, and this thought accompanies a hope, that if it should be my privilege to attend some of your future meetings I may not only profit by your abundant experience, but that I may myself be able to contribute something from my own country which may be interesting and useful to some of your honored members.

In conclusion, I permit myself to thank you most heartily for your personal efforts in my behalf, and beg you to accept my kindest regards with special consideration.

MR. BION J. ARNOLD:—Mr. President, I am very glad, indeed, that the representatives here from foreign countries were brave enough to get up and say something in favor of the three-phase railway, for we of this country have been somewhat slow in this field, and seem here to-day to have pretty thoroughly buried the alternating motor for railway work. I am very sorry to see it. While I may not be ready to advise others to put into practice what I really believe at the present time, and since we are all more or less the creatures of our environments, I might advise differently from what I actually believe, in order to conform to the conservatism necessary for a safe, advising engineer. I am, nevertheless, a believer in the future of the alternating motor for heavy railroad work. I have been, in turn, a friend of the steam locomotive, the storage battery and the rotary converter substation, and have used many of each of them in various plants. For this reason I believe I am in a position to consider the matter from a fair standpoint. I am now in favor of the alternating motor on railroad trains, notwithstanding the fact that it does not seem to have many friends here to-day. How we are going to use it, I am not prepared to say. I am not at all certain that I shall suggest a solution, but I believe somebody will do it. I do not want to admit that the inventive talent employed by the large electrical manufacturing establishments of this country is unable to solve this problem, and I believe if left to itself with a free hand, that it will do it. We have been handling this electrical question, from the railroad standpoint, differently from what I believe it should be handled. The electric motor, primarily, is a constant speeded machine, most efficient at a constant load, and designed to run from a constant pressure. We have been, up to the present time, endeavoring to make it start its load by throwing energy from this constant source, corresponding in hydraulics to a high head of water, into it in great chunks, to use a crude expression, and then letting it dissipate the unutilized energy in the form of heat on itself, or through auxilary resistance, in order to hold back this destructive force until the motor can get

its speed sufficiently high to protect itself. I think this is wrong. It may do for small units, such as we are running street cars with. It may do for electric elevators, where the loads are light. It will not do for heavy trains.

My opinion is that we must in some manner use this, at present, dissipated energy to assist in starting the trains. When we do that, I believe the alternating current motor will pull our railroad trains.

Now, in regard to this paper of Mr. Berg, and the paper of Mr. Armstrong, I want to say that I thoroughly appreciate the amount of work it has taken to give us the information contained in them. It certainly is valuable information. I do wish, however, that Mr. Berg had taken an example of a railroad, which would be a little bit more fair, in my judgment, to the alternating motor. I do not doubt that his intention has been to be perfectly fair, but I do believe that, if he had taken a little greater distance into consideration the figures would have been different. He has taken quite a short road, and a road fairly well adapted to the direct current system. I have no doubt Mr. Berg will answer this, and give us the information I am looking for: *i. e.*, what the conditions were under which these calculations were made, and whether or not a greater distance would have modified these figures. As to his conclusions, for slow speed railway systems such as we are operating in our cities to-day, I think he is correct. I do not think that there is anything that can equal the direct current for such work. Certainly the rotary converter sub-station direct current motor system is better than any other existing plan for interurban roads with light trains, although I believe that the cost of such a system is too great and that it will ultimately give way to the alternating motor system. But when we get into heavy railroad work, where we want to start a heavy train from one terminus and run it through to another terminus at high speed, with few stops, it seems to me the alternating motor is the ideal thing for it. I am looking for the manufacturing engineers of this country to solve that problem.

MR. MAILLOUX:—Mr. President and gentlemen, I have listened with a great deal of interest to the presentation of the papers by Mr. Berg and Mr. Armstrong, and also the remarks which have been made, more especially by my colleague, Mr. Arnold. Mr. Arnold and I had the pleasure of visiting together many polyphase railroad installations in Switzerland last year. We both came home permeated with the idea that the future belonged to the polyphase current. Mr. Arnold, I see, is still quite saturated with that idea. I have had some experience within the last few months which has tended greatly to demagnetize me in that respect. I will not dispute the fact, well recognized, and which has been adverted to by our colleague from Belgium, Prof. Gillon, that for long distances, whether the speeds be high or low,

the alternating current is extremely well suited. It offers many advantages in the generation, the distribution and the utilization of current. There are, however, problems in electric railroading, in cases where it is desired to give a high speed service on relatively short runs, which practically preclude the use of the alternating current motor, in the present state of the art; and I think that in this case one might say that the greater the schedule speed which is to be maintained, the more difficult it is to utilize and apply successfully the alternating current motor. The limitations depend upon the distance run and the speed to be attained in that run.

The curves in Fig. 2 of Mr. Berg's paper give a very interesting comparison, which serves usefully to diagnose the case, so to speak. In this figure Mr. Berg has superposed two diagrams of acceleration, one being the diagram of acceleration of a direct current motor, and the other that of an alternating current motor, both of presumably equivalent capacity and intended for the same service.

The ordinates (y) of these acceleration curves represent speeds, in miles per hour, and the abscissæ (x) represent time in seconds, counted from the instant that the car begins to move.

The rising portions of both curves are shown straight in Fig. 2, although we know that practically they cannot be straight since one of the factors, namely, the train resistance, is not constant, but varies with the speed. In reality, the two curves should both be slightly concave toward the axis of x, although the difference is small, and, for the purpose of comparison, may be negligible. The differential co-efficient of the rising portion of each curve; that is to say, the ratio of any increment of speed to the corresponding increment of time is, in either case, a measure of the change in speed; in this case, of the gain in speed, stated in miles per hour. The straight lines in this case are tantamount to the assumption that the rate of acceleration is constant until the speed line ceases to be straight. In practice it is very nearly constant. The portion of area of each acceleration curve comprised between zero time and any given time is, as is well known, a measure of the distance covered by the car in the corresponding time. The distance covered, when calculated and plotted for each time value, gives the distance curve or integral curve, so-called.

Mr. Berg has given at the upper part of the diagram such a distance curve, which I presume, in this case, is the distance curve corresponding to the alternating current motor. It is to be regretted that he did not also plot the integral curve for the other motor, whichever it was; because it is precisely in the comparison of the integral curves that one learns the most useful lessons in regard to the relative advantages of the two systems of motors, especially for high speed service. If both curves had been plotted, the two curves would show a difference in their rates of rise at the beginning.

This difference in rates of rise of the two distance curves is a necessary mathematical consequence of the fact that the two acceleration curves do not coincide, but rather diverge as they rise from zero; in each case the rate of rise, or the differential coefficient (dy/dx) of the integral curve being directly proportional to the height (y) of the acceleration curve at the same time point (x). This difference in rates of rise of the two distance curves is, therefore, related to the fact, plainly visible in Fig. 2, that the two acceleration curves have humps or crests which do not occur at the same time points.

Now, the two superposed acceleration diagrams, taken with their corresponding distance curves, afford a criterion and comparison of what might be termed the "efficacy of acceleration."

The motor whose distance curve shows the greater rate of rise at the beginning is, of course, the motor which has the more rapid acceleration, for the distance curve shows that it then covers more distance per unit of time. The rapidity of acceleration depends, as is well known, on the steepness of the rising portion of the acceleration curve; in other words, on the angle which the mean direction of the acceleration curve makes with the axis of x in the rising portion, which angle might be called the "initial angle of acceleration," the tangent of which (y/x) is substantially equal to the mean rate of acceleration; *i. e.*, the gain in velocity (miles per hour) per second.

When, as in high speed, rapid transit service, with relatively short runs and high schedule speed, the motor has to make so many starts and runs so little time at full speed that it practically does most of its work on the early part of the acceleration diagram, then, evidently, that motor, and that motor alone, should be selected which can give the greatest initial angle of acceleration, and which consequently soonest can give the maximum rate of rise to the integral or distance curve.

The two acceleration curves in Fig. 2 show quite a divergence in their ascending portions, the "initial angle" of acceleration being greater for the d. c. than for the a. c. motor. This divergence would be much greater still if the diagrams had been made especially for the purpose of comparing or contrasting the time-saving or schedule speed-making possibilities of the two types of motors instead of their economic merits in a case where the schedule speed is inside, although near, the limits of the a. c. motor.

The real reason why the a. c. motor is not as good as the d. c. motor for high speed rapid transit service is simply that the initial angle, or mean rate of acceleration, cannot be made as high as that of the d. c. motor. Its limitation in this respect is due to the fact that the present a. c. motors can develop a maximum starting torque of from two-and-a half to three times the normal full load torque only, whereas a d. c. motor can develop a maximum starting torque equal to five, six, or more times the

average normal running torque. The starting torque of a d. c. motor is, indeed, only limited by the current which one dares to send through it at the time of starting. The curve of tractive effort, as a function of the current, usually shows that the torque increases faster than the current, or that the pull per ampere is actually higher at the limiting current value than at the running current value. As a matter of fact, one may, and does, in emergencies, for very short times, pass larger currents through a d. c. motor than would be usually allowable or necessary, thereby obtaining a tractive effort of as much as eight, nine, or even ten times the mean effort. In the case of the a. c. motor, as pointed out, one is limited, and cannot get a torque beyond that which the impressed e. m. f. can produce. Now, looking again at the two acceleration curves, we see that since the area of the diagram represents distance, the more quickly we can fill up that diagram; that is, the less time we lose in climbing up to full speed the more area we enclose and the more distance we cover in a given time. That is the mathematical reason why we must have an acceleration curve as steep as possible. That can only be obtained by having a motor which will give as large an amount of net tractive effort as possible; that is to say, having a motor which, after having allowed for the tractive effort (pounds pull) necessary to move the car against the axle friction, the rail friction, and the wind friction (*i.e.*, the train resistance), shall give as many *net* pounds as possible, because these only are the ones which are necessary and serviceable for acceleration.

This net pull produces and represents the energy that is to be put into the car in the form of momentum. It must come out of the electric circuit in some form, and I cannot conceive of any way in which we can juggle with the circuit so as to get this energy without expanding it or drawing it from the circuit. Reference has been made to elevator motors, and to means of starting with lower currents. In the case of a car, we have so many tons of mass to which a certain velocity must be given in a relatively short time. That requires a definite amount of mechanical energy (foot pounds) to be generated in the time, and it is therefore the rate at which we can produce those foot pounds and put them into the train that determines the acceleration.

The electrical manipulation of the motor circuits and windings, or the resort to mechanical expedients and accessories like shaft-clutches, gears with variable speed ratios, etc., is only useful to the extent that it improves the efficiency of conversion of electrical into mechanical energy, thus enabling a given motor to produce more mechanical output from a given electrical input; and since the benefit, if any, from these features would be shared by both types of motors, their relative situation would, therefore, not be materially changed thereby. So far, my investigations have resulted in the disappointing conclusion that it is impossible, in relatively short runs, to obtain a

mean angle of acceleration sufficiently steep with the a. c. motor to make it possible for us to hope to produce with it the schedule time required for high speed service. If the run is relatively long it can be done; but I have found that on runs of a mile, a mile and a half, and perhaps more, the difficulties of obtaining a rate of acceleration sufficiently high are so great that the manufacturers give us little or no hope of the immediate solution of the problem. Therefore, while I, myself, started out with the preconceived idea of the great advantages of the alternating current system, and could point as an incentive to very many interesting examples in Europe, yet I am forced to the conclusion that for most cases "the time is not yet." The paper of Mr. Berg, and I think that of Mr. Armstrong, cited cases of mountain railroading where the alternating current system is particularly applicable; but unfortunately these are not the conditions which obtain in the cases which engineers have to deal with at present. They are cases which apply to the future rather than to the present. It will doubtless be necessary for us to demonstrate the feasibility, applicability and practicability of electric railroading on a large scale, on much smaller roads and under vastly different conditions before the roads where the conditions are apparently so ideally favorable can become interested in it.

PROF. GILLON:—I think that I have understood that my friend Mr. Mailloux has spoken of the application of high speeds to lines having frequent stops, such as for metropolitan elevated and underground lines. It seems to me that for short distances, there is hardly any object in exceeding speeds of 60 miles an hour, for the expenditure of energy which would be required for a too rapid acceleration would not be justified by the very small amount of time gained in short distances.

The normal sinuosity (*i. e.*, the unavoidable curves) of said lines will limit, in general, the maximum speed to a speed below 60 miles.

I think Mr. Mailloux has stated that it was possible to obtain direct current motors capable of developing in starting a torque seven or eight, or even nine times the normal running power, whereas the maximum torque of triphase motors does not exceed two and one half times the normal torque.

I admit the existence of such direct current motors, assuming that suitable precautions are taken to avoid the heating of the insulation and sparking at the brushes, but I would call attention to the fact that in the transformer motor car, one can increase at will the starting torque of triphase motors. It is sufficient for this to raise the e. m. f. applied at the terminals of these motors by inserting a few supplementary winding turns. In the secondary of the transformers this is more especially possible, inasmuch as the starting torque increases in proportion to the square of the applied e. m. f.

MR. E. P. ROBERTS:—I desire to ask one or two questions and

to make a few remarks from a similar standpoint to that of Mr. Arnold, who is a designer of electric roads. It has been stated that the polyphase system with induction motors is especially applicable to long hauls at constantly maintained speed. In Ohio, where we have a number of electric railways, we have no such roads. We do not at present have very long roads; *i. e.*, roads of over 50 miles, though we shortly will have; but we do have quite long roads with high speed schedules and comparatively few stops, and one of these roads also operates at the same time a medium speed schedule having a greater number of stops, and this road has only a single track; in other words, the roads are already so long as to require both rapid through service and local cars stopping at all towns and villages, and quite often in the country.

Relative to sub-stations, we are all hoping that it will be possible to do away with these and not have an interest account on the investment in rotating machinery and also a depreciation and repair account on them, and, in addition, the payment of salaries for attendants. Capitalizing salaries and adding it to the cost of machinery makes the total first cost of sub-stations a very considerable percentage of the total cost of an interurban electric railway, and a great increase per mile as compared with ten to fifteen mile direct current roads.

As Mr. Arnold stated, we must look to the manufacturers to obtain machinery to accomplish the results desired, and if they could do away with sub-stations there would be a number of us much pleased; but I have been considerably disappointed, both from the statements made here and lately to us in our office, as to the probability of doing away with sub-stations on the ordinary interurban road, operating under the conditions which generally exist. The amount of power now taken, both average and maximum, and at starting, is very considerable, and if by doing away with sub-stations we must not only increase the first cost of the generating machinery but also of the line work and of the car equipment, it is not improbable that the present system is, in most cases, the better, particularly taken in connection with such matters as being able to operate at variable speed and over existing trolley roads, etc.

The following figures taken from car measurements recently made by assistants in our office may be of interest. They are the results of measurements of 500 miles of run over four roads, each of which has cars of approximately the same size and weight, with four 50 h. p. motors, the difference being mainly in the gearing, maximum and schedule speeds, number of stops per mile and grades of road.

Road No. 1—High schedule and maximum speeds, level road.
" No. 2—Medium schedule speed, level road.
" No. 3—Medium schedule speed, considerable grades and curves.

Road No. 4—Medium schedule speed, heavy grades and curves.

Schedule speed is exclusive of lay-over at end. Running speed is speed when in motion. K. W. is at the car.

	SPEED.			Average stops per mile in country.	k. w. per mile.		Average k.w.	
	Schedule.	Max.	Running.		In country	In city.	In country	In city.
No. 1—	26.2	55	41	.85	2.88	2.11	73.3	21.1
No. 2—	20.5	43	32	1.41	2.85	2.27	56.8	21.6
No. 3—	20.4	39	25	.70	2.18	3.36*	48.5	26.5
No. 4—	20.6	47	24	.56	2.18	2.32	47.8	23.4

*Unusual load and stops.

SPECIAL NOTES—MAX. C.—400 amp. on No. 1—starting, and No. 3 accelerating on grade—Max. grade on No. 3—12 per cent. about ⅛ mile, and taking 15.7 k. w. per car mile.

One of these roads is the one already mentioned where, on a single track, two schedules are simultaneously maintained; *i. e.*, in addition to the regular schedule, the cars of which stop at all towns and villages and a number of stops in the country, through cars are also operated, stopping at only one point between terminal points and not stopping at all in the country.

Relative to locating sub-stations, when designing a new road on which traffic has not been developed, we are confronted with the fact that the fewer the number of cars—in other words, the greater the headway—the more economical it is to have the sub-stations a considerable distance apart, it being cheaper to pay interest on additional feed wire rather than to install numerous sub-stations; but, as the traffic increases, it is preferable to have more sub-stations; therefore, how much to discount the future is a serious question. It is, of course, practicable to change the location of sub-stations, but it is quite an expense, particularly if storage batteries be used.

The designer of electric roads is dependent upon the manufacturer as to what machinery he can obtain to accomplish the desired results. It is not so much a question as to how the results are obtained, as to the certainty of the first cost and cost of operation, and whether the details have been so thoroughly worked out and proved in practice, that they will not only operate with the guaranteed efficiency, but also with reliability.

MR. MAILLOUX:—Mr. President, the remarks which have been made since I spoke show the importance of one qualifying his statements and referring somewhat to' conditions, since, as I pointed out in a previous discussion, one cannot rightly compare things under different conditions. It is, perhaps, necessary for me to state that what I referred to by the expression "high

speed" was not the extremely high speeds which Prof. Gillon adverted to, namely those of 100 to 150 miles per hour, nor yet was it the speeds which are usually referred to and meant by "high speeds" in interurban or suburban work. I referred to speeds which are called high because they seem high by comparison with existing speeds. I mean the speeds of rapid transit service on interurban and suburban lines, that would reach as high as 45 to 50 miles per hour, *schedule* time. These would necessitate a maximum car speed of somewhere between 60 and 80 miles, according to the condition of the road, (the curves, grades, etc.).

I do not wish to be understood as stating that I have lost entire faith in the alternating current motor, or that I have lost all hope of its fulfilling the mission to which Mr. Arnold looks forward with so much hope and confidence. I wish to state that, in many respects, I still regard it as the ideal system, and that I would be pleased extremely to have it shown to me to be feasible for cases of the character I have reference to.

Now, if you again look at the instructive diagram on page 564, you will see that, from the nature of the two curves, the point at which the two distance curves will come together would be somewhere between 60 and 80 seconds; in other words, nearly or only at the end of the run; because they would cross only when the area included between the two rising parts of the curve was equalled by the area of the upper portions of the curve; and that point you can see readily must be somewhere beyond 60 or 70 seconds. I submit that, in most cases in which high speed traction is of interest to us, we must, inside of 70 seconds, have reached the maximum speed at which we are to run, and in many cases we have already reached a speed at which we are obliged to turn off the current; and we readily see that the shorter the run the earlier is the time at which we must shut off the current. Now, if we wish to cover a certain distance in a very short time, it is absolutely necessary that we should reach the top speed in the shortest time possible. In other words, the co-efficient of rise in the acceleration curve (*i. e.*, the gain in speed per second) must be very high. Now, we would doubtless find, by comparing the two integral curves at a point somewhere about 40 seconds from the start, that the alternating current motor would be several seconds behind the other for the same distance. This means that every time we start to go up to a certain speed we are losing a certain number of seconds—5, 10, 15, or whatever the difference is between the two integral curves for the same distance. Now, it is a question whether the conditions of service are such that one can afford to lose those seconds. One can afford to lose them in a case where the schedule speed is not too high; but where everything counts, where it is absolutely necessary to save every second in order to cover the distance and give the people who are to patronize the road a service

of such rapidity and regularity that it will be attractive to them —under those conditions the alternating current motor is "out of the running." The higher the schedule speed at which the comparison is made, the more hopeless is the case of the a. c. motor.

In closing, I would state for the information of Prof. Gillon, that the motors to which I referred as giving a very large starting torque are not of special construction but are of regular standard manufacture. It is not unusual to see a motor, say of about 100 or 150 h.p. geared for 30, 40 or 50 miles, giving a traction of between 2,000 and 4,000 pounds, while the running traction at full speed would be something between 300 and 600; and we can see, as the curves of tractive effort which are published by the manufacturers for these various motors readily show, that it is possible to obtain a very large starting torque. The curves of acceleration built from these data are usually such as to enable one to obtain acceleration at a much higher rate than is deemed safe and practicable. It is perfectly practicable to obtain estimates, that is so far as motors are concerned, for 200 pounds per ton, net, while it is scarcely wise to do it, a lower figure being usually quite sufficient for most purposes. But I have not heard of any case yet where an alternating current motor can show a rate of rise, that is a rate of acceleration which is at all comparable. (The acceleration curve for the a. c. motor in Fig. 2 indicates a net pull of only about half this amount).

MR. STILLWELL:—Mr. President, I have listened with a great deal of interest to the discussion of Mr. Berg's paper, and to the concluding paragraphs of the paper itself. I am sorry I was not here in time to hear Mr. Armstrong's paper, but I shall read that also with much interest. The subject is certainly a very timely one at present. We are putting in railways in the cities and throughout the country, using the direct current, we are extending cross country lines considerable distances, but we have been going ahead on the old lines of established practice, and I am disposed to think that we have not sufficiently, in the last three or four years at least, studied the possibilities of the alternating current in traction work. I think Mr. Berg's paper adds valuable information upon this very pertinent subject. It seems to me, in a subject so broad as this, it is rather unprofitable to discuss it in a sort of general way. There are so many kinds of traction. It focuses ideas if you consider one of the several kinds of traction or of apparatus at a time. The problem of interurban traffic is a very different thing from the problem of traffic in a large city, running with four, five or six car trains, on one minute headway, and it is also a different problem from ordinary street railway practice. As has been stated, the direct current motor has worked out very well in street railway practice, and probably it will permanently survive in that field. When you come to apply it to a road 50 miles long the condi-

tions are different, and we certainly ought to investigate the possibilities of the polyphase alternating current. My feeling is that on interurban roads, where the stops are not frequent, where the speed must be high, the alternating current motor has a field that it will one day occupy. To my mind, one of the great objections to using a power plant with sub-stations to supply direct current to a trolley or third-rail has not been stated, and that objection is the old one of the attendance. You are depending upon three, or four, or five points of supply, and at each of those points you must have attendants upon whose intelligence and vigilance you are compelled to rely for a supply of power. Now, if we could substitute an alternating current system with alternating feeders from one central source of supply, we could concentrate our skill at that one point with better results. There is another limitation of the sub-station plan which comes up very forcibly in large railway work in cities, and that is, in this fact, that you are compelled, if you have half a dozen sub-stations, to have a reserve of machinery in each of those sub-stations. The result is your investment in transforming machinery is out of proportion to the investment in generating machinery. I have in mind a specific case which I have had occasion to figure upon in this respect, that of the Manhattan Railway Company of New York, where the installation will consist of seven 5,000 k. w. alternators to take care of the maximum load, and one for reserve. When you come to the sub-stations, with seven sub-stations the equipment will consist of twenty-six 1,500 k. w. converters, with seven additional for reserve. Now, that figures out a total of 40,000 k. w. capacity installed in the power plant, and 49,500 in the sub-stations.

With regard to acceleration, it is unquestionably desirable in the elevated or underground service of large cities to use high acceleration. This is practically imperative in order to secure maximum speed, and consequently maximum passenger carrying capacity; but there are limitations as regards practical acceleration, especially when the power is supplied through sub-stations, in which case the fluctuating overloads upon sub-station apparatus must be considered. These fluctuating overloads from a large number of machines tend to equalize themselves at the power house, so that the fluctuation there is perhaps only 20 per cent. above normal, but in the sub-stations the fluctuations are relatively much greater.

I have calculated some cases of eight car express trains, running on two minutes headway, where it would seem that the increase of load on rotary converters in a sub-station may amount to as much as 10,000 h. p. in three seconds; now, that is rolling mill practice. Could we use alternating current motors for traction, eliminating the sub-stations, we should avoid this difficulty, which is a serious one in the case of the converter system,

but alternating current for traction purposes must be worked out first under the conditions to which it is best adapted, *i. e.*, cross country work, and I very much hope that the experiments recently undertaken in Germany will throw light upon this very interesting subject.

There is a sufficient reason why no very strong effort to use alternating current motors in the rapid transit service of large cities has been made recently in America, and this reason is obvious if we consider what would be accomplished practically upon the assumption that the alternating current motor could be developed satisfactorily for this kind of work.

The operating expenses of elevated railways in America vary between extremely wide limits, but it is safe to say that the average result is not better than 60 per cent. of the gross receipts. The power bill is about 30 per cent. of the operating expenses, or 18 per cent. of the gross receipts. If we use the rotary converter sub station plan on a large scale, it is not difficult to get an efficiency of at least 80 per cent. from engine to motor. Now, suppose it were possible to eliminate the rotary converters, distribute at 11,000 volts from a central power house to a few sub-stations, there reduce the potential to, say, 3,000 volts, and then operate your motors at 3,000 volts. With efficiency equal to that of direct current motors, you might possibly raise your system from engine to motor to something like a maximum of 90 per cent. This is hardly probable, but assuming that it could be realized, your gain would simply amount to a reduction in your operating expenses equivalent to two per cent. of your income. This result is not a sufficient stake to justify an experiment on a large scale.

In my opinion, also, the risk attendant upon the use of 3,000 volts for traction purposes in cities, except possibly in a tunnel, is far more than offset by any gain in efficiency that can be expected to result by using alternating current motors. The advantage of a polyphase motor system that strikes one at first thought, is the large reduction in plant investment, but when you calculate the saving in operating expenses which would result, could all claims for the alternating current be realized, it seems to me evident that the rotary converter system with direct current motors, as it stands to-day, is the only practical one for rapid transit service in large cities, and that the things promised by the other system are not controlling by any means.

MR. ARNOLD:—I think I stated, Mr. President, when I spoke before, that, as an engineer, I probably would not advise others to do what I believe I would do myself, if I were free to act, by being financially responsible for my results. Therefore, Mr. Stillwell's conclusions as to what an engineer should do, if compelled to act, at the present time are very sound, in my judgment. That does not modify my opinion, though, as to what the future will be. My friend, Mr. Mailloux here, was as thor-

oughly magnetized, as he puts it, at one time, on the subject as I am, and came back from Europe all right. He seems, however, to have listened to the siren voices of some of my friends, and become so enamoured of those curves of his that he has become partly reconverted, and has rotated to the direct current. I think, however, that he must repent as time goes on. I also admit that, for city work, our present street car system for short distances, and the rotary converter sub-station scheme for long distances, are all right up to the present time. I think the conclusion of Mr. Stillwell is, that it is the best up to date. I agree that this is the safest position to take. I do not believe, however, that for long distance roads the present plans are going to hold. It has been stated recently by a prominent railway official that he can pull freight cheaper by electricity than by steam, and he has been operating an electric freight road and passenger road for some five years. I am very much gratified to have that statement made, and by so good an authority. If that statement is true (I confess that it comes as a surprise to me), we are going to pull a good deal of freight and a good many passenger trains by electricity, and, when that time comes, I believe we will pull them with alternating current motors. These curves of Mr. Mailloux do not worry me as much as they once did; I have probably seen hundreds of such curves, had them made in my own office, and watched the peaks on them, much to my dismay, go too high for comfort, and have tried many ways for overcoming the difficulty caused by them. I do not say how this will be done; I only say it must and will be done, by the engineers of some country, and I do not like to see the electrical engineers of this country lie down and admit that the best thing on earth for railway work is the direct current, and that we, therefore, have perfection.[1]

MR. E. P. ROBERTS:—Mr. Stillwell mentions the attendance in sub-stations. I would state that in a recent preliminary investigation, relative to a considerable length of road, we divided the estimate into two parts:

1st—The cost of producing power at the switchboard in the power house, and

2d—The cost of transmission from such point to the d. c. feeder switchboards in the sub-stations, including first cost, depreciation and repairs, and interest, and adding also, for certain reasons, the capitalization of the salaries in the sub-stations. Our transportation charges were, if I recollect correctly, something more than 50 per cent. greater than the cost of producing energy. I would add, however, that the source of power was a very cheap one. Of course, if we could have estimated on static transformers instead of sub-stations, the cost of transportation of energy would be very much reduced.

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1 See page 306 for communication made after adjournment.

Mr. Stillwell also mentioned excessive momentary loads in sub-stations. We have a case where three cars operating at high speed give fluctuations at the d. c. switchboard of from 0 to 1,000 amperes, sometimes exceeding the latter amount. Where there are such tremendous fluctuations, the question of the installation of storage batteries should receive careful consideration.

PRESIDENT STEINMETZ:—The opinion expressed by Mr. Stillwill, I have heard a number of times, that the three-phase induction railway system would offer over the direct current the advantage of a higher transmission efficiency, and with less reserve apparatus and less operating expense; but, more than that, I not only had that opinion, but carried it out in practice by building such an equipment—that was in 1894, seven years ago—and had a very large number of experiments carried out with it. Mr. Armstrong, who has read the paper, has been in charge of this general investigation and experiment. Since that time we have gone over and over these things again, with another equipment, and another equipment, and were willing even to go to the risk of installing, without any commercial profit, and merely to establish the practical experience in commercial operation, the apparatus for a three-phase railway system, not a very large installation, but an installation of moderate size, where the loss, in case of its inferiority to the continuous current system, would not be considered so serious. But upon more careful investigation my opinion always was reversed, and I came to the conclusion that the transmission efficiency of the three-phase system is lower than that of the continuous current, for the reason that the efficiency of the induction motor in service conditions is so very much lower than that of the continuous current series motor, that the loss in the motor more than makes up for the lesser loss in the transmission line and for the continuous loss in the converter.

Then there is another question that has been spoken of, the enormous fluctuations of load in the sub-stations. Now, if you go to the three-phase induction motor these fluctuatons of load are very much larger, much more intensified, because these fluctuations are due to starting at excessive torque, and the torque increases with the current at a much faster rate with a continuous current series motor than with the induction motor, so that by using the three-phase motor and getting rid of the converter the fluctuations are very greatly intensified. If you could get rid of the static transformer and use only the converter, then it would be all right, because the converter is the piece of apparatus which is least affected by these fluctuations. It will run all right if the load varies 100 or 150 per cent. of a full load. Of all electric apparatus, the stationary transformer is that which stands overloads the least. The objection to overloads, temporary overloads, is the heating of the apparatus. Now, if you take the loss curve of electric apparatus, the increase of loss with the output, you will

find that in the stationary transformer it rises at a very much greater rate than in either the converter or the continuous current generator, or the alternating current generator, or the synchronous motor. Only in the induction motor does it rise just as fast. So that you see, the weakest link in the present system is not the converter but the transformer.

As regards reserve machinery, the elimination of the converter in very many cases causes a very much greater reserve capacity, and not only reserve capacity, but reserve capacity continuously in circuit, in the form of stationary transformers. In such a transformer system, to get fair operation, a very small line loss is essential, so that the voltage does not drop much, even with the excessive starting current of the motor. Since the motor torque varies with the square of the current, to get, with a considerable drop in the lines, a sufficient margin of starting torque, such an excessive overload capacity of the motor has to be provided, that under normal running the motor is vastly underloaded, and therefore its efficiency and power factor almost evanescent in service conditions. To avoid this, transformers have to be located at frequent intervals, which means, that frequently the total transformer capacity in such a system is two or three times the generator capacity, more or less. In this case, as in most others, there are certain conditions for which one apparatus is suitable, certain conditions for which the other is suitable; but the higher efficiency of the series motor system is brought out by numerical investigation in almost every case. And then there is the question of attendance. That may appear serious for the first moment, but the rotary converter has been developed to such an extent that it requires extremely little attention. It requires some attention. You have to look at it once a day, or every few days; but you must consider that on these roads you must have stations and ticket agents, and you must have probably, where the speed is very high, a block system, and the men employed there may, every day or so, look at the converter and see whether it has oil in the bearings, etc.; so, as a rule, there is no additional charge for attendance on the rotary converters. Now, all that depends on the circumstances of the individual case. I may add to Mr. Arnold's remarks, that this particular instance mentioned in Mr. Berg's paper, which he criticised, I believe is taken from an actual proposal, and a proposal in which the three-phase and the continuous current systems came into fierce actual competition.

MR. ARNOLD:—I want to ask one question of President Steinmetz. I think I know the case. Why was the alternating bid half a million dollars less than the direct current?

PRESIDENT STEINMETZ:—Well, I don't know why it was. But, you see, sometimes something new—any new apparatus, as a rule, is very much more expensive than an old one; but sometimes it is offered very much cheaper, because people

do not always realize what they are running into; and while they make allowances for all sorts of things, they may underestimate the allowances. I know of instances of that kind. I have been caught that way myself by making an allowance of 100 per cent. and finding I ought to have allowed 500 per cent.

I would like to hear from some of our English cousins, who come from a country where the question of the relative merits of the alternating and continuous current in railway service has been brought to general attention. I hoped to hear from them before in this discussion, and we should like to hear from them now.

MR. A. H. POTT:—Mr. President and Gentlemen: I am sorry that although I come from a country in which this question has been debated very hotly of late, I can add nothing to the discussion of any value. We at present are looking into the question, both on the continent, in Germany, Austria and on this side, to see what we can learn. At present, so far as I can make out, there are such diverse opinions on both sides of the water as to the merits of this system that I think the best we can do is to shut our mouths until we find from actual practice which is the right solution. The Italian railway, I understand, started yesterday, or a few days ago, and perhaps that will give us some information. But, of course, there is a very strong feeling at present in England that we want something which, for large schemes, is at least cheaper and more elastic than the present systems. I can only say that of the bids which were made in connection with what I suppose is in the minds of all—our underground railway—the bid was very much lower for the alternating system than any other. I mean not just low enough to show a slight difference, but enough to catch the eye of any engineer. That being so, it was only right to look into the matter very carefully. I had hoped to see a very satisfactory demonstration of the high tension alternating system, but I regret to say the financial conditions of the line have led to the railway passing into other hands, and I am afraid we shall see it done by one of the existing methods after all.

MR. SCOTT:—Mr. President, in the very interesting and able papers which we have heard, we have, I presume, not merely the opinion of the individual writers, but rather the concensus of opinion of their colleagues. They have had the facilities of a large manufacturing company, and, as Mr. Steinmetz has just intimated, the efforts and experiments of a large company for several years in this line, and it is presumably the digested result of this work extending over several years by the able engineers of a large company which we have in these papers. It detracts nothing from these papers, and would, I think, only add to them to say that it also confirms the conclusions of other engineers who have been working along the same lines. In fact, within

the last few months I have prepared a statement somewhat on the line of these papers, and at one time contemplated working it up for this very meeting, but, very fortunately, I have been saved the trouble, as my contribution would probably have added but little to what has already been presented. The conclusions which, in connection with my colleagues of the Westinghouse company, I have arrived at, after having this subject pretty closely in mind for a number of years, are similar to those which have been stated in the papers which have been read. Not only as to general conclusions, but in particular. We, too, have been figuring on some special cases, and some figures have been given which I recognize as very similar to those which we have had under consideration. Mr. Berg's paper takes a specific case and states the premises, the methods and the conclusions. One of his conclusions is that the alternating current motor running in concatenation would take 26 per cent. more energy than the direct current motor. I happen to remember a corresponding figure, which was 24 per cent. The increase of generating capacity (neglecting reserve) which Mr. Berg finds for the alternating, is about 66 per cent. in the generator and about 20 per cent. in the engine. Those figures are within a few per cent. (I do not recall the exact per cent. now) of the conclusions which we reached. This bears also upon another point which was brought up a moment ago, the saving in power by using the alternating current system. The saving in engine power is a negative saving, amounting to about 20 per cent. the wrong way, if our conclusions are right in this particular case. I have not noticed whether in this paper the increased weight of the alternating motors and rheostats was taken into consideration. You took that into consideration?

MR. BERG:—Yes. I think about 65 cent.

MR. SCOTT:—Braking has been referred to in the paper as giving but little advantage in a case of this kind. In general statements which are made with regard to the advantage of employing induction motors, it is stated that if used in concatenation they may be used in braking, thus saving a large amount of energy, theoretically 50 per cent of the stored energy—practically something less than that, possibly 30 per cent. That point was investigated quite carefully. It was found that if the motors are used for braking, they must naturally be worked harder and be of larger size. The consequent increased weight would very nearly compensate for the energy which might be returned. Moreover, on many runs it is feasible to rise quickly to a high speed, drift, and begin working at a low speed, and in that way most of the energy which is stored at the higher speed is returned very efficiently to the service by keeping the car going and overcoming friction. In order to get the greatest advantage in braking, it is necessary to begin braking at the highest speed. The energy which might be returned to the line, in the form of electric

power, could, therefore, be used in a very much better way by changing the conditions a little, and simply using it for overcoming friction by drifting. The electric braking cannot be done under half speed, and it will in very many cases be possible to drift down to this speed, or even lower.

The concordant results of the two large manufacturing companies that have considered this matter are further substantiated by the results which have been attained by consulting engineers. Take the large roads in New York City, including the Manhattan elevated. The actual result there, after an examination extending over years, in which the matter was fully canvassed by the large manufacturing companies and by the Manhattan Railway Company, led to the use of the direct current motors. That, then, seems to be the American engineering practice for this particular line of work. Foreign engineers have taken the lead in a somewhat different direction. I had the opportunity to read over the abstract of a report which had been made within the last few months regarding the operation of a large road which seems to be in the minds of a number of us, and while I will not attempt to take it up point by point, there are one or two features which I now recall. One was the statement, that the induction motor is superior to the direct current motor, because it does not slow down on grades. It keeps the speed right up grades, and therefore serves to maintain the schedule. My own way of looking at that characteristic of the motor is that it is susceptible of entirely different conclusions. As has been pointed out in the paper which has been read, if we go up grade at the same speed with alternating and direct current motors, then on a level our alternating current motor is limited to practically the same speed, while our direct current motor could run at a much higher speed. It was stated in this report, also, that the weight or cost of the alternating current motor was practically the same. As the American engineers point out, however, in that particular case, it was proposed to have half of the motors on the train idle above half speed, thereby certainly adding to the motor weight. It was proposed also to use motors in braking. We are a little at a loss here to see just what kind of motors are put in competition with direct current motors. It certainly cannot be the kind of motor which has been contemplated in these papers and which we know about over here. In that same report it was stated that a large number of alternating roads in Europe were in operation, the bare statement going without elaboration, and the inference could readily be drawn that all these were of the same class. I think that some of these roads have one car shuttling back and forth, and others have cars going up mountains, neither of which situations would apply to a city road. While it may be hard for us to understand the position of the foreign engineer, the foreign engineer is in a like predicament in regard to us. He is in difficulty to understand just our

position. I had the good fortune to have a talk on this subject with one of our esteemed foreign visitors a few days ago. After talking a little while, he said that he had really been unable to understand the position of some of the American engineers. He had seen these alternating current roads run, and the cars start, and start quickly, run smoothly, and all that sort of thing, and yet when he had gotten our worthy President at one side and had a talk with him, why, he really seemed to throw cold water on the scheme, and the water seemed, in fact, very cold. He was at a loss to understand why this was. The thing itself seemed to be all right, but he couldn't understand the American engineer. He did the American engineer the compliment not to attribute that lack to his ability, but rather, possibly, to his integrity. He thought possibly it was that, being in connection with a manufacturing concern, the commercial department of that concern repressed the idea, and it was better policy to use the direct current and not make new patterns. I refer to that for one or two reasons. The first thing I did in reply was to resent immediately that position: that opinions given as engineering opinions by a distinguished engineer of a manufacturing company would be so distorted and controlled by the other departments of his company. We hold a reputation as engineers which we want duly respected. In the second place, I think that he is entirely mistaken in assuming that the commercial departments or the management of the larger companies want the alternating system repressed. I think they have put it up to the engineers pretty emphatically, and that it is the engineers, through the results of their tests and investigations, that have retarded the work rather than the opposite. If all this be true then we have a pretty good consensus of American engineering opinion and practice here, where we have been noted for our advanced stand in railway work in other lines, where it was to be expected we would know what is what, and what is best, and be looking out for the best; and the general results should be a pretty weighty argument with those who are taking up the subject. It is true there is now, and there always has been, a sort of feeling that electricity is wonderful, and the alternating current is magic. If you can't do it any other way, the alternating current is the thing that has got to come in and do it somehow. The lesson of the last few days has been that each kind of apparatus has its own legitimate field. There are extremes in conditions where one thing is best and the other will not do, and *vice versa*. A few days ago we considered the alternating current motor and the direct current motor; a little later, the induction motor and the synchronous motor; and to-day, the induction motor for railway work and the direct current for railway work. Now, no one statement will cover the whole ground. There are extremes in which each has its own proper place, and there is a wide middle ground, and our discussions seek to locate the dividing line. Where is the dividing

line? What are you going to class one way, and what the other? Now, in treating the induction motor and the railway motor, it is not necessary to take into consideration all the peculiarities of alternating apparatus. It is really not a comparison so much between the alternating and the direct current as it is between the two types of working, between the series motor and the shunt motor. If you cannot use a direct current shunt motor with constant field, without varying its field strength, in any particular case, you can depend pretty surely upon it that the induction motor would not do either. The paper under discussion is fortunately quite definite. It is no imaginary and vague statement, but conditions are definitely laid down, and the conclusions drawn are with respect to certain definite kinds of work; and, whatever the future may have in store, I myself am personally quite convinced that the conclusions here drawn with regard to the inapplicability of the induction motor to this class of work are correctly drawn.

The paper will, I am sure, be of great value in giving the results of this kind of work and showing the conditions and the characteristics of this class of apparatus to many of our engineering friends who have not had the facilities for work and investigation in this line.

MR. POTT:—Mr. President and gentlemen, I wish to make a personal explanation, although it may detain you a few minutes. I think that the English cousin referred to by Mr. Scott was myself, and I should be extremely sorry if any American engineer or member of this INSTITUTE thought that I had in any way said anything that was in the slightest degree discourteous to your President. It was quite the last thing I should have said. What I meant when I made those remarks, and I am quite prepared to withdraw it if it is explained to be incorrect, although it was first put into my head by one of your own members, was that in this country, what I think you call the supply business, did to a certain extent control the expressed opinions of the engineers; that is to say, that if the engineers were told they could not get a thing, that was sufficient; for the present they could not get it. But I had no intention whatever of suggesting that any remarks Mr. Steinmetz made in what I termed throwing cold water on the three-phase system, had any reference to his connection with a supply company, and I hope that this is quite clear. I think you will excuse my saying that in almost any country manufacturers are always apt rather to "gas" on things which they are fully prepared to supply to the market than on those things which they are not.

PRESIDENT STEINMETZ:—Gentlemen, regarding the last statement, as the rule the tendency with large companies is rather the opposite, and justly so. While it may be very nice and convenient, especially in hard times, just to build what you have built before, and not experiment, at the same time, for a large

company, the most serious matter is to be left behind, and it is of first importance, then, always to be leading; and for that reason, if there is any new method, any new problem, that comes up which even faintly promises to offer a prospect of success, it is just the large companies that are willing to investigate it and to put all their energy into finding out exactly what there is in it. Most propositions, however, which appear very encouraging at first, when carefully investigated lose their promising nature, and then very naturally the one who made the proposition is very likely to think that it is only conservatism which opposes the introduction of new, revolutionary ideas; whereas, in reality it is because they are inferior to what exists. For instance, with regard to this very thing, since 1894, when I made the first experiments, I have been trying to install a three-phase railway system, and I have enough influence with the company with which I am connected, that we were willing to give it free of profit, at cost price, if we only could get the three-phase railroad installed in commercial service. In several cases we almost got it, but always at the last the customer concluded after carefully investigating the matter and consulting with us, that really there was no excuse for him to take it, even at cost price, and so we have not been able yet to build any three-phase railroad, except one which we are now building for northern Italy; but still, as has been brought out in our discussion, there are certain conditions of operation which are suitable for the three-phase railroad, and I hope to live long enough to see a railroad equipped under those conditions with three-phase motors; that is, a railroad of some magnitude, not with one or two cars climbing a mountain or shunting to and fro, but a real railroad system installed on the three-phase plan under conditions which are eminently suited for three-phase, and more suitable for three-phase than for direct current.

MR. STILLWELL:—Mr. President, just a word. It has been pointed out that in my remarks a while ago I did not refer to the effect of the motor efficiency upon the efficiency of the system as a whole. I had in mind the efficiency of the motor, assuming that the manufacturers would be able to work out motors which would be regarded of the same approximate efficiency. I was considering simply the question of distribution, which, of course, when you come to constructing, is to be considered as a separate matter. But there is a point with regard to the alternating current motor which is alleged to be a possibility and a practical thing by a very influential and well-informed firm of manufacturers in Europe; that is, they return a very substantial amount of energy to the system. Now, the importance of that depends upon the kind of service you are talking about. For service like that of the Manhattan railway of New York, or underground, of these Chicago railroads, where you have 60 seconds between stops, or are accelerating

for 25 seconds, and drifting for perhaps 25 more, and putting on the brakes for ten—the direct current motor is not an ideal or final consummation as we use it. With the conditions of efficiency of the direct current motors with their rheostats it does seem to be a fault that with a third rail, with 86 per cent. of energy to start with from the switchboard of our power plant, we have to kill 35 per cent. of that amount before we can get it into the shape of power for traction, the net result being about 50 per cent. of the energy you start with at the switchboard being utilized to draw the train.

I have been very greatly interested in the fact that the representatives of the two manufacturing companies in America who have given most attention to this subject are in close agreement as to the possibilities and limitations of the induction motor. At the same time I wish to express the hope that they are not going to bury that question. It would be a great pity if three years from now, after we have adopted, perhaps, the continuous current system on the New York underground, we should have it pointed out to us that a firm of Budapesth had put in in London a system cheaper to install and equally effective and efficient in operation. We must not take that risk.

PRESIDENT STEINMETZ:—Gentlemen, I am sorry to say that this great advantage claimed for the three-phase induction motor, to return energy in concatenation during retardation, is also one of the features which careful numerical investigation does not bear out, for this reason; if you run practically at synchronism until the moment the brakes are put on, that is, if you operate the motor in a manner which is improper and wasteful, you can return energy; but, as Mr. Mailloux has very well brought out, the proper way of operating a motor under rapid transit conditions, that is, the way to carry the motor in a given time over a given distance, with the minimum expenditure of energy, is to accelerate as fast as possible to high speed, then to shut off the power, and coast until it is time to put on the brakes, because thereby you devote to necessary operation a large part of the energy otherwise dissipated in the brakes, and it is naturally more efficient, instead of wasting energy in the brakes, to use it for carrying the train over the friction of the road. Now, in the three-phase induction motor, where the maximum speed is a little below synchronism, and you begin to coast at this speed, at the end of coasting you have fallen down from a little below synchronism by the total energy expended on the friction of the road, and you are fairly close to half speed, and the motor can return energy only above half speed. That means the range over which it can return energy becomes very small, and the actual amount of energy returned in this case under average conditions, even with very short stops, where you cannot coast for a very long time, is a very few per cent., so few that it does not pay commercially to use it, because the energy is returned at the

expense of an enormous amount of current which passes through the motor and heats it. So the induction motor as an electric brake for returning energy is not suitable for rapid transit work. It is all right to use the motor in concatenation to start and get up to half speed; but even then it is inferior in efficiency to series control of continuous motors. Two concatenated motors are lower in efficiency than two continuous current motors in series, the current of the first motor being in excess. So that even this claimed advantage of the polyphase induction motor in returning energy is not borne out by experience.

MR. STERN:—As to the suggestion of Mr. Stillwell regarding long runs, it appears that there has not been much, if anything, said about running the induction motor for long distances, about 50 miles, for express service, or distances approaching that, say 20, 30 or 40 miles, and it occurs to me that under such conditions as that there might be something said in favor of it. The induction motor appears to have its analogy, in mechanical engineering, in the gas engine. Regarding their relations to torque and speed, the acceleration of both have small values. There are mechanical means, however, as has been stated by Mr. Scott, for varying the torque and the acceleration while the speed of the motor is kept constant, which are employed in the driving gear of gasoline motor vehicles, but on long runs, such as I have just mentioned, it would appear that the motor would take care of that itself, particularly on level roads, and the reference that Mr. Arnold made some time ago to the possibilities of the induction motor for railroad travel on long runs (trunk lines) might have some points of merit, and those points it appears have been overlooked in considering urban and interurban traffic when the issue was one of express service. That is the point I believe Mr. Arnold wished to bring out.

MR. H. C. SPAULDING:—Noting the suggestion that has been made about using mechanical means for obtaining speed variation with constant speed motors, it may be of interest to the members present to note a device which I have had occasion to use more or less in machine design, and have found most efficient for the purpose. Suppose (referring to blackboard sketch) on either the driving or driven shaft, a pulley having a deep V-groove, and split through the bottom of the groove into two parts, one of which is fixed to the shaft, the other being adjustable by any convenient means, so as to vary the distance between the two conical faces thus formed. In this groove is used a narrow, thick beveled edge belt which drives by side contact only, and does not bottom except when it is desirable to combine a starting and stopping function with variable speed—in this case a loose pulley being inserted between the two faces of the groove pulley, which are recessed for the purpose. By using this device on both driving and driven pulleys (the take-up on the latter being

effected automatically by a spiral spring), a variation of speed between wide limits is obtainable with maximum efficiency, and entire absence of excessive pressure on bearings. At the Columbian Exposition in Chicago I ran something like an average of 15 h. p. from the main shafting by this device, and, while such economy is not to be expected in every case, in this instance only a half horse-power was charged for, as all computations were made by the inspector on a basis of belt width. In this case the belt was 2 inches wide, oak-tanned, with staggered rivets, and I believe a similar mechanism could be used to advantage in solving some traction problems. One application is fully described in United States patent No. 596,281, issued to me on December 28, 1897.

MR. HOLMES:—Mr. President, there was a company that exploited this speed-varying device applied to a car. Some of those present from Chicago may know of it. I have seen drawings of such a car intended for suburban work, but it never went further than the experimental stage.

In electrical engineering, Mr. President, there is a firing line of battle; there are not many of us engineers who are working on that line. The representatives of two or three large manufacturing companies are truly in the battle fray, but most of us are handling problems that are not experimental. I was very much interested, therefore, in the percentage which Mr. Stillwell gave, showing the deterioration of machinery in the Cataract plant. Here is a great hydraulic electric plant, in fact, the pioneer in this country, which has been running for eight years. He tells us that the cost of electrical repairs on it is less than two per cent. If I recollect rightly, this is less than half the cost of repair upon its hydraulic machinery. Coming down, then, to the alternating station, where electricity is used in its various forms, we find the apparatus there, also, of lasting quality and extremely satisfactory. Now, is it not plain to us that the revolving field, alternating current generator, therefore, with the efficiency which it now has, is a thing that we can recommend permanently as a machine which does represent the final thing along the lines of electric generation of power with magnetism and electricity? It seems to me that it does. I, therefore, have pictured to myself the problem which is sometimes stated: If, for instance, between New York and Albany, you divide the distance, or travel half the distance, and then travel, or divide the distance again, and then divide it again, how long will it take you to reach Albany? Practically infinity. But in this development of the three-phase alternating generator and its connections, have we not got very far along toward Albany, and can't our recommendations to people who wish to buy be made with great confidence? It does not seem to me as if the alternating current generator will become obsolete in the life of this generation, and it seems to me as if its lasting quality is assured. I was also

interested in the statement concerning the Falls plant, that if they were building over again, or were building *de novo*, the apparatus which they would put in would not secure a saving of more than $1 per kilowatt a year. That is something very interesting, considering the fact that this problem was proposed eight years ago.

MR. STERN :—I do not think that is very encouraging to engineers, Mr. President. If we were to stop where we are and continue building the same machinery that we are building, engineers would not have very much to do in the future.

MR. MAILLOUX :—Mr. President, I think that before this discussion ends some allusion ought to be made to another phase of the electric traction problem which, though not specifically mentioned in the paper, is none the less one of the most important factors in the problem now before us. I think that an occasion of this kind is suited to bring to the attention of the profession some of the points on which we are still in the dark. One of the things on which we most need information, at present, is data relating to the effect of the air and the wind on the resistance of trains. Ten years ago it was a matter of little consequence, because the speeds at which we ran cars were so low that the wind friction was a negligible quantity. The greater portion of the energy expended in propelling a car at full speed; in fact, nearly the whole of the energy was expended in overcoming the friction of the bearings; but to-day, when the speeds have reached 30, 40, 50, 60 miles, and when speeds of 100 miles or more per hour are contemplated we are reaching a condition when the rolling friction of the train becomes the negligible quantity in its relation to the air friction. It seems rather strange at first blush to think that in a train running at 65 miles an hour, more than two-thirds of the energy is expended in a rubbing of some form or other against the cars of the train, yet it is a fact. We are, unfortunately, almost unprovided with data on this question, although these data are destined to have a great influence on the solution of many problems. The absence of these data is responsible to-day for the fact that we can only give approximate solutions and approximate results of calculations, as given, for instance, in the curves, in the valuable paper of Mr. Berg. The curves relating to drifting or coasting, are all straight lines, whereas we know that they cannot be straight lines. The curves of acceleration must be concave, while the curves of drifting must be convex, to the axis of ω; yet we have no means of determining definitely and precisely in what manner the departure from the straight line exists. In some cases, especially at the higher speeds, that departure is so great as to become of the highest importance indeed in the result. I have seen instances lately of speed-time curves which were made on the assumption that the train friction was a constant,—12, 13, 15 pounds per ton, for instance. These curves might be correct enough if the speeds remained under 15

or 20 miles per hour; yet when the maximum speeds run up to 30 or 50 miles per hour this assumption is far from true, for the train friction would run up to 25 or even 30 pounds. Under such conditions you can readily see that the curves are hypothetical and are only a rough approximation, and unless the motors have a great deal of margin in capacity for starting current, unless it be possible to raise the potential of the line or in some other way to get higher speed, conditions might arise in which the theoretical schedule could not be made when it came to actual practice. Now, in order to solve this problem, and like problems, it would be necessary to have data, and data, and data of all kinds, from all sources and under all conditions, and it is especially to the engineers who are designing and installing traction plants of all kinds that I wish to appeal. I think that efforts should be made by all engineers to procure and collate data on this subject of train resistance; it does not make much difference under what conditions, provided the conditions are noted. It will be necessary, of course, to sift out the data; but if the data themselves were only available there are many engineers who are competent to do the sifting, and who would be only too glad to do so. In that way we could eventually arrive at some definite, accurate information that would be of the greatest value. Efforts have already been made by engineers, but they have been isolated to some extent. It is very desirable, indeed, that a sort of joint effort should be made by all engineers who have the opportunity in connection with the plants in which they may be interested, as supervising, constructing or operating engineers, to procure and make supplemental data. I will here state that I am greatly indebted to the General Electric Company for some data on train resistance at high speeds, which they themselves have obtained in a particular case. These data turned out to be of extreme utility to me; and while they apply only to this case, or to cognate cases, yet but for these data, I would have been almost entirely in the dark as to the assumptions that one should make in regard to the effect of wind resistance at high speeds; and I assure you, gentlemen, that we are likely to hit far off the target if we have not some means of guidance in making assumptions in such matters. It is for that reason that I deem the matter of so great importance, and I think it ought to have proper and systematic attention. One of the mooted questions at present among engineers is whether the wind pressure causes expenditure or loss of energy by the boring effect of the train, or by the skin friction of the sides of the train. I had a very interesting conversation with our colleague from Belgium, Prof. Gillon, the other day on the subject, in which he related the conclusions of the German engineers, and I think it would be quite interesting if Prof. Gillon would state something further in regard to the concensus of opinion of continental engineers on that question.

MR. ARNOLD:—Mr. President, pending the collection of such

data as Mr. Mailloux proposes, I suggest a simple formula which I learned some years ago, and which, I believe, was proposed by the *Engineering News*, after quite a prolonged discussion of this subject in its columns by its readers. It is: $R = \frac{1}{4} V + 2$, where R equals the total train resistance in pounds per ton and V equals the velocity in miles per hour, while 2 is a constant determined by experiment.

This is short and sufficiently accurate for practical purposes for speeds up to 50 miles per hour.

MR. MAILLOUX:—Mr. President, I submit that the rule is over 100 per cent. out of the way at 50 miles per hour.

MR. ARNOLD:—I submit, Mr. President, that I do not think it can be proved so far incorrect at or under the speeds to which I have limited it.

MR. MAILLOUX:—The rule is at least 100 per cent. out of the way, perhaps 200, at 60 miles.

PROF. GILLON:—[*Translation*] What I have said to Mr. Mailloux is a mere expression of opinion. In my opinion, that which plays the essential role in the resistance which the air opposes to a moving train vehicle is the friction of the air against the walls of the vehicle. If this be true, the nature of this wall and the care that should be taken in making it as smooth as possible would assume great importance. On the other hand, the sharpened form sometimes given to the front end of high speed cars would become merely accessory, and it would seem to be preferable, if the above hypotheses be true, to utilize this space in the front of the car for the motorman compartment.

It may be questioned that if we should allow the wind to blow through the trucks and motors beyond the extent required for ventilation, whether it would not be better to cover the parts below the vehicle, so as to reduce the wind resistance. These are questions which experience alone can answer, and of which we can hope for a solution as soon as there are high speed electric trains in operation.

MR. E. P. ROBERTS:—Mr. President, early in the discussion you made a statement that sub-stations could sometimes be placed in towns where a ticket and baggage agent could also look after the machinery, and I noticed that several smiled as if this were not a practical proposition. In our experience, it is a matter which should always be taken into consideration, as, for example, in one road which we designed it was a question whether to place two sub-stations in the country, one each side of a town, in which town it was also decided to have a ticket, baggage and freight agent, or whether to place a sub-station in the town. Although the latter somewhat increased the first cost, the increase was mainly due to the increased feeder wire, which has practically no depreciation and repair account, and the cost of the machinery was actually less; and, capitalizing the differences in salaries, the result was materially in favor of the one sub-station, even though

the resulting distance between the sub-stations was in excess of what would ordinarily be considered good practice.

PRESIDENT STEINMETZ:—If there are no further questions, I will call upon Mr. Armstrong and Mr. Berg to conclude the discussion.

MR. BERG:—After Mr. Scott's discussion of my paper, there is nothing I have to say particularly. I would only like to explain to Mr. Mailloux, that in calculating these curves I very carefully took the variation in resistance into consideration at first, and afterward drew the curves on the basis of average resistance during acceleration. I found the amount of departure, I think, one per cent., and I thought to save all discussion I would simply draw a straight line, as the difference was so small it was impossible to detect it. Regarding the distance-time curve, when calculating that curve, I actually put in dots and dashes for two curves, but afterward drew one line, because they came so close together in this particular scale that if I tried to draw two curves they would have been so close the difference would hardly be noticeable. Had the maximum speed, however, been say twice as high, Mr. Mailloux's arguments might well apply.

MR. ARMSTRONG:—Mr. President, I think it is an extreme disappointment to several of the members here that the induction motor has not proved itself worthy of installation in some of our more important plants, and the person more disappointed than any other, I believe, is our President. You can make very beautiful curves in induction motor calculations, and to the mathematician, especially, it is a source of delight, and for that reason more than any other I think our President is seriously discouraged at the failure of the induction motor to prove itself worthy of more careful consideration. It has been brought out by the discussion to-day that the induction motor is unsuitable for city traffic, rapid transit work, elevated work, or, in fact, any class of service where the stops are at all frequent. Without going into the details of what has been said, I would add simply that I agree with them thoroughly, but I would like to point out that rapid transit work is not confined to city limits. On the suburban roads, although they may have distances of two or three miles between stops, the schedule speeds may be so high that they constitute another class of rapid transit roads; the conditions governing distribution, and, in fact, of the system as a whole, are very nearly those governing the city roads, which are classed as true rapid transit roads. The cars are continually accelerating, they drift, and brakes are applied, very similarly to the way it is done in city work, except that the intervals are longer. And, therefore, in bringing out many of the statements in my paper, I had in mind that interurban roads and suburban roads, and more especially those of the future, called for such extreme qualifications of the motors that the induction motor is not at all suited for the work.

MR. N. C. SAWERS:—Mr. President and Members of the AMERICAN

INSTITUTE OF ELECTRICAL ENGINEERS:—I want to take this opportunity before adjournment to thank you all very much indeed for your great kindness and most generous hospitality to us during our visit here. Our visit will always be a most pleasant recollection to all of us, and the next time you come to England—and I trust that day is not far distant—although we cannot promise to provide such sumptuous intellectual fare as we have enjoyed here, still I hope you will be able to pick up some titbits here and there, and, from the social point of view, enjoy yourselves as heartily and meet as thoroughly good fellows in our little country as we have in your big one.

PRESIDENT STEINMETZ:—Gentlemen, I think I voice your sentiments when I say that we have not the least doubt that when we go over the great pond to our cousins on the other side, we shall be received as well as is imaginable and shall have a very nice time; and I hope that we shall once in the future meet over there again and enjoy ourselves just as well as is possible.

Mr. Pope, our Secretary, desires to make some more announcements.

SECRETARY POPE:—This will be very brief, gentlemen. I have a telegram from Mr. Stanley, of Great Barrington, Mass., in which he requests that the INSTITUTE be invited to visit Great Barrington next year on the occasion of its Annual Convention. This matter has been considered by some of our members, and I can say, from personal knowledge of the ground and the facilities, that it offers every advantage for a very successful and pleasant convention, and I hope in due time the Board of Directors will take it into consideration.

PRESIDENT STEINMETZ:—The program of our Convention is herewith closed, and I thank you very much for your very generous attendance on the meetings and for the interest and participation you have shown in the discussions, which I feel confident have made this Convention one of the best we ever had. As regards attendance, it certainly has been, and even as regards social entertainment, I believe we have not lagged behind previous Conventions, although unavoidably some of the surroundings have not been quite as appropriate as we could have desired.

I believe a motion to adjourn the Convention is now in order.

On motion of Mr. Hering, seconded by Mr. Arnold, the meeting adjourned.

[CONTRIBUTED AFTER ADJOURNMENT BY C. O. MAILLOUX.]

In my previous discussion of Mr. Berg's paper, in referring to the diagram at the top of Fig. 2 of his diagrams, I stated that if the distance curves for the a. c. motor and for the d. c. motor had both been plotted together, it would be found that these distance curves do not coincide at all points, but show a percep-

tible time interval between them at certain points. The accompanying diagrams, Figs. A and B, prove that this is actually the case. These diagrams have been prepared by means of an integraph, an instrument which not only computes, but actually draws the integral curve. The two curved lines in each diagram represent the two integral curves corresponding, respectively, to the a. c. and the d. c. motors, exactly as they were drawn by the integraph when the tracing point of the instrument was run successively over the two speed-time curves, namely, the one for the a. c. motor and the one for the d. c. motor, shown on Fig. 2 of Mr. Berg's paper. In Fig. A the setting of the instrument was such as to preserve and reproduce exactly the same scale for both the ordinates (distance traversed in feet), and the abscissæ (time in seconds). In Fig. B the scale of abscissæ is also the same as in Mr. Berg's diagram, but the scale of ordinates has been purposely magnified to a little over twice the scale of ordin-

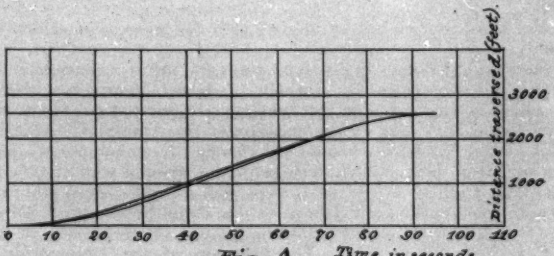


Fig. A.

ates in Fig. A. In both figures the lower line of the curve is the integral or distance curve corresponding to the a. c. motor, and the upper curve is that corresponding to the d. c. motor. It is seen that in each diagram both curves reach the same height (corresponding to a little over 2,500 feet) at the end of the run, in the same time (about 95 seconds), showing that the a. c. and d. c. speed-time curves as drawn in Mr. Berg's diagram (Fig. 2), are really curves of equal area, as intended by Mr. Berg. The divergence between the lines shows that the d. c. motor (upper line) during the middle part of the run reaches a given point; that is to say, runs over a given distance a little sooner than the a. c. motor, being about 2.5 seconds ahead at the point of maximum divergence. The actual time interval or difference is the same in both diagrams, but is more plainly visible in Fig. B, owing to the enlarged scale of ordinates.

As may be inferred from my previous discussion, this divergence would be still greater if an attempt had been made to accel-

erate faster in both cases, the rate of acceleration being quite low for both motors, in Fig. 2 of Mr. Berg's diagrams, thereby favoring the a. c. motor.

During the first 20 seconds, or for the portion of time during which the acceleration line is straight for both motors, the mean acceleration in Fig. 2 is about 0.8 of a mile per hour per second for the a. c. motor, and quite nearly, but a trifle under, *one* mile per hour per second for the d. c. motor. It would be found that the acceleration in the case of the a. c. motor can only with great

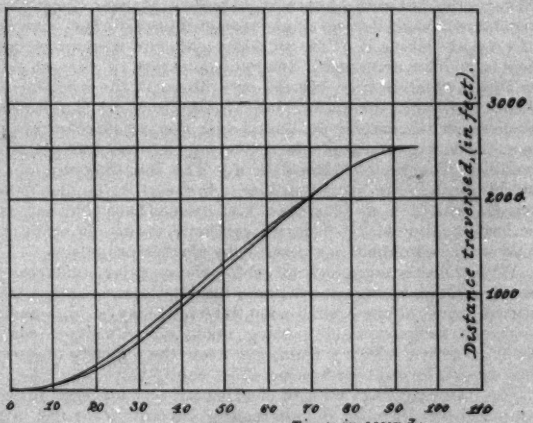


Fig. B.

difficulty be made to exceed 1.5 miles per hour per second, while it is not at all difficult to attain an acceleration of at least 2.5 miles per hour per second with the d. c. motor, if desired. For a rapid transit system having relatively short runs and a high schedule speed, and, therefore, requiring a maximum speed of say 50 or 60 miles (instead of about 27, as in Mr. Berg's diagram), a very great divergence would necessarily result between the distance curves corresponding to an a. c. motor equipment and a d. c. motor equipment for the same service, for the reason that the mean acceleration in the case of the d. c. motor would be equivalent to about two miles per hour per second, as against about 1.4, or possibly, 1.5 miles per hour per second, which is practically the present limit for the a. c. motor.

[Communicated after Adjournment by B. J. Arnold.]

During the discussion I was unable, on account of certain causes, to give fully my reasons for believing in the ultimate supremacy of the alternating current motor for railway work, so I would like to supplement my remarks somewhat.

It is well known among those who have to do with electric railways that we have just about reached the limit of train weight if we must confine ourselves to the present accepted maximum direct current voltage of 650 to 700 volts. Beyond this the commutator and insulation difficulties with the direct current motor seem to be too great to overcome. If confined to this limited voltage, the quantity of current necessary to haul a heavy train becomes so large that no single contact shoe or wheel is capable of properly taking it off the working conductor without excessive heating. This necessitates the multiplication of contact points or shoes, which in time become prohibitive if the working conductor is placed overhead, and if the working conductor is placed near the ground, as is usual with the present third rail system, the voltage must necessarily be kept low on account of our inability to completely insulate it. The low pressure on the working conductor necessitates large first cost in the distributing system, which, in my judgment, is at present excessive, and must be largely eliminated before we can hope to handle heavy railroad work commercially successful by electric propulsion.

With the alternating current motor, the working conductor can be placed high enough to properly insulate it, and energy transmitted over it at any desired potential which may be high enough to permit sufficient energy being taken off on a single contact point to propel a heavy train and keep the quantity of current low enough to make its heating effect negligible.

The present contended lack of efficiency and low rate of acceleration charged against the alternating current motor are, in my judgment, less objectionable and more easily overcome than the objections I have pointed out.

[DISCUSSION OF PRESIDENT STEINMETZ'S PAPER ON "OSCILLATIONS OF EXTREMELY HIGH POTENTIALS IN ALTERNATING HIGH POTENTIAL TRANSMISSIONS."—COMMUNICATED AFTER ADJOURNMENT BY PERCY H. THOMAS.]

[See p. 705, Oct. 1901 issue, for Mr. Steinmetz's paper.]

Mr. Steinmetz has presented a very interesting paper, and, as a result of his investigation, has reached some rather unexpected conclusions. The third of these conclusions, which are summarized at the end of the paper, is particularly unexpected. It is this portion of the paper which I wish to discuss. If it be true that a short circuit may cause a rise of potential beyond the strength of available insulating materials, the matter is of great importance and deserves the most careful consideration. The basis from which the author arrives at this conclusion is equation (39) of his paper. I do not consider all the assumptions made in the derivation of this equation to be justifiable, as I will explain.

The differential equation (3), which is the mathematical basis of the whole paper, expresses the fact that in the circuit under discussion the instantaneous value of the impressed e. m. f. always equals the sum of the ohmic drop, the back e. m. f. due to self induction, and the e. m. f. on the condenser. The potential on the condenser has previously been expressed as the integral of the current flowing in the circuit divided by the capacity; that is, the current considered in the equation is the charging current to the condenser. The differential equation evidently applies to the phenomena of charging a condenser through a circuit containing inductance and resistance. The author derives from it a general equation for the current in the circuit and e. m. f. on the condenser. The solution of this problem has been given by others as well, for example by Bedell and Crehore in their book, "Alternating Currents." They start with practically the same differential equation and derive a result (see page 127, equation 181) the equivalent of equation (13) in Mr. Steinmetz's paper. In equation (35), preparatory to deriving equation (39), the author substitutes for the initial current of his general equation the current flowing in the circuit on a short circuit across the lines at the condenser. This substitution is justifiable only on the assumption, which the author directly makes, that the short circuit current is instantaneously interrupted at the point of short circuit and that no diminution in the strength of the current in the rest of the circuit occurs at the time of this interruption. This assumption, which is the step in the investigation that I do not consider justifiable and from which results directly the great rise of potential of equation (39), is certainly not in accordance with actual conditions in transmission circuits. For not only do such short circuits fail to open instantaneously, but when accompanied by an arc the current strength is considerably limited before breaking by the resistance of the arc. Also any excessive rise of potential will cause the short circuit to be re-established at the original point. Furthermore, if a similar assumption of a sudden interruption of the current is made in any electric circuit containing inductance, a rise of potential will result.

The rise of potential derived from equation (39) is the well known phenomena of a high potential resulting from the sudden stoppage of current in a circuit which includes inductance. That is, when the short circuit occurs, energy is stored in the inductance of the transmission line by the short circuit current and on the assumption of the instantaneous rupture of the current, the energy can be discharged only into the condenser, which of course means a rise of potential depending on the amount of energy stored and the capacity of the condenser. In fact, the expression $\sqrt{k/x}\,E$ which the author gives as a mathematical expression of the extreme rise of potential under discussion, may be directly derived, algebraically, without the use of a differential equation, provided the same assumptions are made as in the paper. For, in a circuit of inductance L, in which a current i_0 is flowing, the energy stored in virtue of the inductance is $1/2\ L\ i_0^2$. If the assumption is made that the current i_0 which is taken to be the short circuit current, is instantaneously interrupted at short circuit this current can discharge only into the condenser, which, must therefore, receive the full amount of this energy. The energy stored in a condenser equals $1/2\ CV^2$ where V is its potential. Equating these two quantities and solving for V, we have the equation $V = i_0\ \sqrt{L/C}$; or substituting for C its value from the paper,

$$\frac{1}{2\,\pi\,N\,k} \text{ and for } L \text{ its value } \frac{x}{2\,\pi\,N},$$

we have $V = i_0\ \sqrt{x\,k}$

Again making the assumption as in the paper, that $r = 0$, $i_0 = E/x$, therefore $V = \sqrt{k/x}\,.\,E = e$, the result given by the author. That is, by obtaining the value by which the potential of the condenser must be raised to contain the energy stored in the inductance of the circuit, we get the author's expression for the rise of potential under discussion.

The fact that the rise of potential results from the author's assumption of his initial conditions may be shown by placing $E=0$ in his general equation (17), for the voltage on the condenser that is, by assuming that no external electromotive force is applied to the circuit after the time, $t=0$. By his assumption, $e_0=0$ in the case of a short circuit. Making $e_0=0$ and $E=0$ all terms vanish except the term

$$\varepsilon^{-(r/2x)\phi}\left[\frac{r^2+q^2}{2\,q}\right] i_0 \sin\frac{q}{2\,x}\,\varphi = \varepsilon^{-(r/2x)\phi}\,\frac{4\,k\,x}{2\,q}\,i_0 \sin\frac{q}{2\,x}\,\varphi$$

Making the assumption again that $r = 0$, we have $i_0\ \sqrt{k\,x}\ \sin\ \sqrt{k/x}\,\varphi$, and for the maximum potential as before $i_0\ \sqrt{k\,x}$, or substituting the old value for i_0 we have $\sqrt{k/x}.E$. That is, the

same maximum rise of potential is obtained if we have no applied e. m. f. in the circuit after the time of rupture of the short circuit. This shows that the assumption of his initial current i_0 introduces the critical term.

Returning to the third conclusion of paper, we see that the rise of potential depends directly on the assumption of a sudden break of the short circuit current. Therefore, anything that tends to render the opening of the short circuit more sudden will increase the danger of an excessive rise of potential That is, in the absence of direct experimental evidence to the contrary from actual transmission circuits or their equivalent, the assumption will naturally follow that these types of circuit opening devices, which have special forms for getting a quick break—for example, magnetic blow-outs, explosive types, air blasts or breaks under oil—are more likely to cause a dangerous rise of potential than the open air type.

In conclusion, I may state that this paper is an interesting and important contribution to the subject of switching in high tension circuits. Unfortunately the mathematical treatment can show nothing as to the vital question as to how sudden a break may result from the various types of switches and circuit breakers. Also the fact should be recognized, which is not directly stated in the paper, that the expression obtained, apparently showing the possibility of an extreme rise of potential, is not due to a new or mysterious principle, but is a particular case of a well known principle in electrical work.

Associates elected at a meeting of the Board of Directors, held Friday, June 28th, 1901.

ASHE, SYDNEY WHITMORE	Assistant in Physics, Polytechnic Institute; residence, Webster Avenue, Bedford Park, Brooklyn, N. Y.	Samuel Sheldon. Wm. J. Hammer. Ralph W. Pope.
BAKER, WILLIAM EDGAR	Chief Electrical Engineer, Manhattan Railway Co., 32 Park Place, New York, N. Y.	W. B. Potter. Henry G. Stott. E. W. Rice, Jr.
BAUSCH, FREDERICK EMIL	Chief Engineer, Pittsburg Plate Glass Co., Crystal City, Mo.	E. L. Nichols. H. A. Wagner. H. J. Ryan.
BICKNELL, DANA EDWIN	Assistant in Inspection Department, Western Electric Co., 57 Bethune St; residence, 2 Perry St., New York, N. Y.	Geo. A. Hamilton. H. F. Albright. G. F. Atwood.
BRIESEN, HAROLD V.	Student in Electrical Engineering, Columbia University, N. Y.; residence, 80 Hackensack Ave., Hoboken, N. J.	F. B. Crocker. F. Townsend. M. I. Pupin.
BROWN, BENJAMIN	Student, Polytechnic Institute, residence, 381 Sackett Street, Brooklyn, N. Y.	Samuel Sheldon. Wm. J. Hammer. Ralph W. Pope.
COLLINS, ARCHIE FREDERICK	Electrical Engineer and Inventor, Collin's Wireless Telephone and Telegraph Co., 132 S. 6th St., Philadelphia; residence, Narberth, Pa.	H. W. Blake. T. C. Martin. W. D. Weaver.
GILMAN, FRANCIS LYMAN	Telephone Engineer, Western Electric Co., 463 West Street; residence, 132 East 16th St., New York, N. Y.	O. A. Bell. P. G. Burton. G. F. Atwood.
HALL, HARRIOTT CURTIS	Glen Cove, L. I. Student, Polytechnic Institute, Brooklyn, N. Y.	Samuel Sheldon. Wm. J. Hammer. Ralph W. Pope.
LOBO, GUSTAVE	Office Engineer, V. M Braschi & Bro., Apartado No. 830, Mexico City, Mexico.	F. B. Crocker. T. C. Martin. Wm. J. Hammer.
MACCALLA, CLIFFORD SHERRON	Foreman Construction, Edison Electric Illuminating Co., 360 Pearl St.; residence, 11 St. James Pl., B'klyn, N.Y.	W. S. Franklin. W. S. Barstow. H. A. Lardner.
MASON, HOBART	Graduate Student, Polytechnic Institute; residence, 432 Greene Ave., Brooklyn, N. Y.	Samuel Sheldon. Wm. J. Hammer. Ralph W. Pope.
MCCARTY, FRANCIS ALEXANDER	Electrical Engineer, Noyes Bros., 31 O'Connell St., Sydney, Australia.	Chas. F. Scott. C. E. Skinner. W. K. Dunlap.
PITCHER, FRANK HENRY	Chief Engineer, Montreal Water and Power Co., 62 Imperial Building, Montreal, P. Q.	R. B. Owens. L. A. Herdt. R. T. Lozier.
SCHROEDER, HENRY	Inspector, Edison Electric Illuminating Co.; residence, 137 Madison Ave., New York, N.Y.	Arthur Williams. Douglass Burnett. Warren D. Brown

SONN, GEORGE C.	Professor of Physics, Newark High School; residence, 285 Belleville Ave., Newark, N. J.	Ralph W. Pope. Wm. J. Hammer. F. E. Kinsman.
STEVENS, CABOT	Electrical Engineer, Brooklyn Edison Electric Illuminating Co., 360 Pearl St.; residence, 11 St. James Pl., Brooklyn, N. Y.	W. S. Barstow. W. L. R. Emmet. Calvin W. Rice.
THAYER, WM. CURTIS	Draughtsman, The Natural Food Co., 11 B. St., Echota, Niagara Falls, N. Y.	Chas. F. Brush. C. A. Adams. E. A. Deeds.
WILSON, ROBERT LEE	Constructing Engineer, Westinghouse Electric and M'f'g. Co., residence, 95 Clark Street, Brooklyn, N. Y.	W. S. Barstow. Chas. F. Scott. W. K. Dunlap.
WOLFE, JOSEPH THOMAS	General Engineer and Sales Agent, Noyes Bros., 15 Queen St., Melbourne, Victoria.	Chas. F. Scott. A. J. Wurts. J. S. Peck.
WOODWARD, FREDERICK SEARLE	Inspector, Edison Electric Illuminating Co. of Brooklyn, 360 Pearl St.; residence, 265 Prospect Pl., B'klyn, N.Y.	W. S. Barstow. E. F. Peck. E. R. Knowles.

Total, 21.

Associates Transferred to Membership:

Approved by Board of Examiners, May 10th, 1901.

BARTON, PHILIP PRICE	Asst. Supt. Niagara Falls Pr. Co., Niagara Falls, N. Y.
CHILD, CHAS. T.	Technical Editor, Electrical Review, N. Y.
GARFIELD, ALEX. STANLEY	Engineer, Cie Thomson-Houston, 27 Rue de Londres, Paris, France.
HILL, GEORGE	Consulting Engineer, 150 Fifth Ave., N. Y.
PERRY, JOHN	Royal College of Science, London.
PINKERTON, ANDREW	Electrical Engineer, American Sheet Steel Co., Vandergrift, Pa.
SETHMAN, GEO. HENRY	Mechanical and Electrical Engineer, Skinner & Sethman, Denver, Col.
SPARKS, CHAS. PRATT	Chief Engineer County of London Electric Lighting Co., Surrey, Eng.
SWINTON, A. A. CAMPBELL	Consulting Electrical Engineer, 66 Victoria St., London, S. W., England.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, Sept. 27, 1901.

The 156th meeting of the INSTITUTE was held this date at 12 West 31st Street, and was called to order by Prest. Steinmetz at 8.30 P. M.

THE SECRETARY :—At the meeting of the Board of Directors this afternoon the following associates were elected :

[See p. 786, Oct. 1901, issue.]

Dr. Matthews then presented the following paper :

[See p. 671, Aug. and Sept. 1901 issue.]

DISCUSSION.

THE PRESIDENT :—Gentlemen, we have listened to a very interesting paper by Dr. Matthews describing a simplified method for photometrically determining arc lamps. I am quite sure there are many amongst you who are interested in the topic of photometry, especially of arc lamps, and who have given this matter a good deal of attention, so I hope that we shall have a very animated and interesting discussion.

I know that Dr. Sharp has given the matter of photometry a good deal of attention. We should be very glad to hear from him.

Discussion is now in order.

DR. CLAYTON H. SHARP :—I think, Mr. President, that everybody engaged in the electric lighting business in any way is very greatly indebted to Dr. Matthews for the work that he has done in this extremely troublesome subject of arc light photometry. As Dr. Matthews said in the opening of his remarks, the subject has rather fallen into disrepute; but that is probably the fault of the very imperfect methods which it has been necessary to follow in the past. The improvement which has been described in this

paper is an enormous one. The apparatus is of a comparatively simple nature and enables us to do what it has been almost impossible to do before; that is, to get truly reliable and close measurements on arc lamps.

I wish to ask Dr. Matthews if he has made any experiments which would indicate that it is possible to substitute for the rotating sectored disk a sheet of glass smoked after the method which he has described. Might not such a sheet of glass serve for cutting down the intensity of the light in a simple and equally effective method? And I should like to inquire also if a reduced copy of this same arrangement could not be made which would be equally applicable to the determination of the mean spherical candle power of the incandescent lamp by means of one measurement.

DR. MATTHEWS:—I can say, in reply to Dr. Sharp, that it is quite possible to cut down the light by means of the smoked glass and do away with the rotating sectored disk. There is nothing, of course, to prevent your smoking the horizontal sector quite heavily; then the others in a sine ratio. In that case you do away with the necessity of using the rotating disk. It is better, probably, to use the smoked glass, and I may say that I have done so with entire success. I have also in the laboratory just the device that Dr. Sharp speaks of, a miniature equipment like this for incandescent light photometry. Of course, when one comes to the photometry of incandescent lights, he no longer has to deal with a source whose photometric surface is one of revolution. By that I mean that the distribution of intensity through any given vertical plane is not the same as that through any other vertical plane. With the arc, the average distribution is constant in all vertical planes. Hence, with the incandescent lamp, one must spin the lamp to get the mean effect. The little instrument that I have operating in my laboratory for glow lamp work is provided with a rotator, and, except that I use smoked glass for cutting down light instead of the rotating sectored disk, is exactly like this apparatus described here. In the case of incandescent lights, there is not the necessity of cutting down the intensity that there would be in the case of the arc, so I presume that answers the question.

THE PRESIDENT:—Well, gentlemen, I am sure there is more to be said on this subject, and I should like to hear something further.

MR. C. J. SPENCER:—I would ask about the difference between the distribution of candle power in the alternating current and direct current lamps, I understand there is a difference.

DR. MATTHEWS:—Yes; there is a very marked difference, of course. Perhaps I can indicate it on the board. The upward lobe of light from the alternating arc is the same as the downward lobe, whereas from the direct current lamp the downward lobe is much greater than the upward, assuming the positive car-

bon to be above. However, with an enclosed arc you cannot predict what the distribution is going to look like. I think, perhaps, that the curves I published two years ago in the National Electric Light Association's report bring that point out. The contour and density of the opalescent globes, and the presence of reflecting surfaces in the lamp, modify the distribution. If you look at that report you will be struck by the fact that there seems to be no very uniform type of distribution. However, when we take the mean of all these curves from different lamps—twenty or twenty-five of them—we get a very smooth and regular curvature which may be taken as characteristic, I think, of the enclosed arc lamp.

MR. GEORGE T. HANCHETT:—Dr. Matthews has rescued arc light photometry from one of the worst starts any system of measurement ever had. I have no doubt that we all remember the day when they used to add north, south, east and west candle power together to make up 2,000, and this certainly is quantitative, to say the least. If I understand Dr. Matthews's apparatus correctly, it seems to me subject to a great deal of refinement when the necessary facilities are at hand for constructing something more elaborate. Dr. Matthews has had to experiment with flat pieces of glass because he could get them, and he chose a a large number of them to make the sigma equations as nearly as possible represent the integral sign. Now it seems to me that this method is of sufficient value to justify the attempt to make a mirror which shall be the frustum of a cone instead of the frustum of a pyramid. From Dr. Matthews's results as to the absorption of the different methods and from his statements it appears that the same piece of glass produces substantially the same absorption, and as the mirror was all of one piece, of course this error would be corrected and the necessity for the sectors of variable absorption in front of the screen would be done away with, and the continuous surface of the mirror would make the number of mirrors, as you might say, infinite, and the result correspondingly refined. We can twist glass into some very intricate shapes for cases where the demand is very small. In this case, it seems to me that the demand is such that we ought to get conical mirrors.

DR. MATTHEWS:—There is one serious objection, of course, to that. As soon as you depart from a plane mirror you get a focusing mirror. Now all these mirrors do is to give you the effect of 24 arc lights instead of one. If you used a curved mirror you would have a focusing effect, and what you would measure would be not intensity of light but flux of light. You would be collecting the flux of light and concentrating it. It is just like using a lens instead of a piece of plain glass.

MR. HANCHETT:—Do I understand that holds true of a ruled surface, one on which a straight line may be drawn? When a surface is curved and has curvature in three directions I can see where it might make some difference; but it seems to me it is

simply a case of multiplying the number of mirrors. As long as you take the plane mirrors you can multiply them indefinitely according to the theory, if I understand it correctly. It is simply carrying it to the limit.

DR. MATTHEWS:—Not quite; you cannot go above a certain size, unless you increase the size of your diameter. You must see as you stand at the photometer, in each mirror, the image of the lamp in its entirety. Now if you multiply the number of mirrors and do not increase the diameter of your ring, then you will see only part of the lamp in each mirror. Of course that won't do. So really we do not need to think of the mirror at all. It is simply as if we had an arc lamp for each mirror, but set at different angles. Now as you increase the number of the mirrors, you would have to increase the diameter of the ring in order that you could still see in each mirror the complete lamp. I think I am right about that.

MR. HANCHETT:—The effect on the screen is the joint effect of all those reflections, and it seems to me the light from the conical mirror, even if it be a reflected image, ought to blend the results together in the same way.

DR. SHARP:—There is another difficulty connected with that proposition, and that is, it would be necessary for the smoking on the glass to be so graduated as to cut down the luminous intensity proportionately to the sine of the angle, continuously; a hard thing to get.

MR. HANCHETT:—That would be impossible, of course.

THE PRESIDENT:—Gentlemen, the arrangement devised by Dr. Matthews essentially consists in taking a simultaneous reading of the intensity of the arc lamp in the different directions and by the method described, by smoking the glass, integrating individual readings after multiplying by the sine. Here is one point that rather appears to me somewhat complicated; that is, the necessity of having the screen smoked exactly to a certain intensity of absorption. That can be done in the laboratory, but it is rather a delicate task. I believe it would be preferable if some method could be devised for doing this multiplication of the individual intensities before integration, mechanically, and not by absorption.

There is another question I would like to ask Dr. Matthews. How would it operate if the mirrors were replaced by white surfaces, to measure the illumination, as a secondary source of light; that is, instead of a circle of reflecting mirrors have a circle of white disks, and measure the total illumination coming from these disks?

DR. MATTHEWS:—I think we would then run into this very same difficulty I have spoken of, that we would be measuring this light flux, or what is sometimes called a quantity of light. In other words, it would be a question of how big these white surfaces were, since by making them smaller or larger we could

get a different effect at the photometer. In other words, the white surface simply diffuses the light. The effect that you are going to get at the photometer would be dependent, I think, on the size of the diffusing surface.

THE PRESIDENT:—But, again, shouldn't you expect if you have white surfaces at unit distance from the arc lamp and collect the total light flux issuing from those white surfaces, that you could from this reduce to the intensity of the source of light?

DR. MATTHEWS:—I think it would be possible. Only, then we would have to take into account the dimensions of the white surfaces. That is really the principle of Blondel's lumenmetre. It is based on the principle of collecting a certain portion of the flux of the light and concentrating it upon the photometer screen. But you have to know the relation between the diffusion of a definite surface and the quantity of light which that surface receives, and it is this fact which complicates Blondel's apparatus. That is the very effect that I attempted to avoid by using plane mirrors. We cannot, it seems to me, too clearly understand that the mirrors do not need to be thought of in the apparatus at all. It is just as if the mirrors were not there. You have simply 24 arc lamps turned at different angles. That is the aspect as you stand at the photometer and look towards the system. You do not really see any mirrors. You see 24 arc lamps. They are tipped up at various angles just as this picture shows, and especially Fig. 4, which, I think, shows it very well. They are arranged in a hemi-circle, in order that the photometric condition may be the same for all.

THE PRESIDENT:—By using the same light-reflecting surfaces for the standard source of light also, would not that eliminate all the difficulties with the coefficient of diffusion of the white surfaces? Assuming that you do not compare directly the arc lamp with your standard incandescent lamp, or whatever it is, but compare the light diffused from a number of white surfaces at some distance from the arc in a circle around it, and the light issuing as diffused light from the white surfaces cut from the same material at a certain distance from the standard lamp, would not that eliminate the constants of diffusion, absorption, etc.? The reason I refer to that is this: It appears to me it would make it possible to eliminate the adjustment for the sine of the angle by having the area of these white diffusing surfaces proportional to the sine of the angle. While we then have to bring into the calculation the dimensions of the apparatus, it seems to me we could eliminate all coefficients of absorption, etc. I do not know whether that would be feasible, but I should like to hear.

DR. MATTHEWS:—I think it is feasible in part, but there is one serious objection. That is, that you have, in the usual photometric process to move either the photometer or the standard light. Now, as you move the photometer, you of course change the solid angles which each one of those surfaces subtends. It can easily

be seen that that would complicate matters. In the use of a diffusing surface, the effect, of course, is dependent on the dimensions of the surface, since it is a question of how much flux that diffusing surface receives from the source, and an apparatus constructed on the plan that the President suggests would have to take account of these dimensions. Now, if we are to eliminate the size of this by having the same state of affairs around the standard lamp, it is difficult to see what we would do when we have to move the standard.

THE PRESIDENT:—Couldn't you get over this difficulty by not moving the standard lamp, but moving the point of observation between the two? Then both distances will change, but you can get over the whole range then.

DR. MATTHEWS:—I would not attempt to say that it is not possible, although I am afraid that the complications would be even worse than they are in the device that I have described.

DR. SHARP:—I would like to suggest, in regard to the President's proposition, that if we had a lot of diffusing surfaces receiving light, they would throw light one upon the other, and we would receive from each surface not only a certain definite fraction of the light which it receives from the source proper, but also a considerable luminous flux which is due to secondary reflections, reflections from the other diffusing screens. That is entirely done away with in the case of the plane mirrors properly constructed.

THE PRESIDENT:—I think you might get around that even by having one diffusing surface revolved around the light and giving it such a motion that the angle of incidence will vary. The reason why I refer to this is that as a rule it is much easier outside of the laboratory to determine mechanical dimensions, distance and size than it is to measure coefficients of absorption. So that where you do not have the facilities of the laboratory at hand, any way of eliminating the necessity of determining coefficients of absorption and adjusting them is desirable.

MR. HANCHETT:—I should like to ask about the mean diameter of this frustum of a pyramid forming the mirror.

DR. MATTHEWS:—About six feet. I thought I could give you the exact measurement, but I am afraid I cannot. The total distance is 865 centimetres, and the diameter is not less than two metres.

MR. HANCHETT:—It is rather difficult to revolve a plane surface in such a circle as that, is it not?

DR. MATTHEWS:—Yes.

MR. WOODWARD:—I should like to ask in regard to that diagram. Those numbers are made up, as I understand it, from readings taken at stated intervals of time, are they not?

DR. MATTHEWS:—Yes.

MR. WOODWARD:—Has there ever been any attempt made to have a self-registering record?

DR. MATTHEWS:—I am afraid there never will be anything but the human eye to fix those values.

MR. WOODWARD:—Could not the selenium cell be used?

DR. MATTHEWS:—That has been attempted, but the question is, of course, with the selenium cell, what is its relation to the eye? The selenium cell and photographic effects and all other effects differ from the physiological effect.

MR. WOODWARD:—The thought in my mind was that there might be some fixed relation between the two, and by making the comparison to start with, you then would have a record to look back to and save the time of the observer.

DR. MATTHEWS:—Yes, one would naturally think of that. Of course, though, such a calibration would have to be made for every different source that was used, for I think the quality of the radiation would affect it.

THE PRESIDENT:—Gentlemen, if there is no further discussion we are through with the topic of to-night's programme. The next meeting will be October 25th, and we expect then to introduce a modification of our present custom; that is, to have not one paper but several short papers dealing with the same topic and have these papers printed and distributed several days beforehand, so that the paper will not need to be read *in toto*, but only in abstract, thereby giving more time for discussion, and I expect that we shall have a very complete discussion then, since there will be several papers treating on the same matters. The subject will be announced in due time.

[COMMUNICATED AFTER ADJOURNMENT BY A. E. KENNELLY.]

Professor Matthews's interesting paper illustrates the importance in practical photometry of employing an apparatus which at a single observation will supply the mean spherical candle power of a source of light. This is a somewhat difficult problem when the source of light, instead of being a small crater, as in the simple voltaic arc, or within a relatively small space, as in the incandescent lamp, occupies a large volume, as in a globe-covered arc lamp. Professor Matthews's solution of the problem in this case consists in mounting in a vertical circle containing the lamp a series of mirrors at 15° angular intervals, of such size to throw light upon the photometer screen from 24 parts of the lamp globe (or 22 parts in practice since the top and bottom mirrors can be omitted), and inserting an absorbent screen in the path of each mirror, in such a manner as to absorb the reflected light in proportion to the cosine of the altitude. As this gives rise to a 22-fold reflection, with a resultant total reflection of about 15-fold intensity on the photometer screen, a sectored revolving disk is used to subdue the measured illumination. Although the preparation and adjustment of such an apparatus must be somewhat laborious, there can be no doubt of the advantage which accrues from the use of the apparatus once in adjustment,

since a single observation gives a close approximation to the mean spherical candle-power, and therefore to the total emission of light.

In the comparison of light sources the spherical candle-power is the only reliable basis. Just as mean horizontal candle-power is a fairer basis of comparison than horizontal candle-power in a single direction, so spherical candle-power, or total light emission is a fairer basis of comparison than mere mean horizontal candle-power, or light emission in a horizontal band. At the same time it seems doubtful whether it would be advisable to rate lamps commercially in terms of spherical candle-power. The mean spherical candle-power of an incandescent lamp is usually from 80 per cent. to 90 per cent. of its mean horizontal candle-power, while in an arc lamp it is commonly from 0 per cent. to 100 per cent. in excess of the mean horizontal candle-power, but only 30 per cent. to 60 per cent. of the maximum candle-power, upon which the rating is ordinarily based. Consequently, to change the rating of lamps to the basis of mean spherical candle-power, although a gain in relative accuracy, would be commercially objectionable, because it would diminish incandescent lamp ratings about 15 per cent., and arc-lamp ratings about 50 per cent. The change, moreover, would be likely to bring about confusion between the two different ratings. It would seem preferable to refer spherical candle-power measurements to the amount of light emitted. Since a unit spherical candle-power represents 4π lumens of light emitted, a spherical Hefner means 12.566 Hefner lumens emitted, a spherical British candle-power 12.566 British lumens, and so on. Consequently, a 16-candle power incandescent lamp, instead of being reckoned as a lamp of, say 13.6 mean British spherical candle-power, would be a lamp of 170.9 British lumens, and if consuming 50 watts would have a light efficiency of 3.418 British lumens per watt. Similarly, an arc-lamp of 1,200 maximum British candle-power, instead of being rated as of, say 600 mean British spherical candle-power, would be a lamp of 7,540 British lumens, and if consuming 450 watts its light efficiency would be 16.75 British lumens per watt.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, Oct. 25, 1901.

The meeting of the INSTITUTE was held this date at 12 W. 31st street, and was called to order by President Steinmetz at 8:25 P. M.

THE PRESIDENT:—Gentlemen, will the meeting come to order. I give the floor to Mr. Pope, the Secretary, to make some announcements.

SECRETARY POPE:—Mr. President, at the meeting of the Executive Committee and the Board of Directors this afternoon, twenty-six associate members were elected. Their names are as follows:

Name	Address	Endorsed by
ABBOTT, WILLIAM L.	Chief Operating Engineer Chicago Edison Co., 139 Adams St.; residence, 3213 Beacon St., Chicago, Ill.	James Lyman. Samuel Insull. C. M. Wilkes.
ALVERSON, HARRY BARTLETT	Superintendent, Cataract Power and Conduit Co.; residence, 40 Court St., Buffalo, N. Y.	D. C. Jackson. H. W. Buck. H. G. Stott.
ATKINS, CHARLES GILMAN	Engineer on Power Business, Chicago Edison Co., 139 Adams St., Chicago, Ill.	James Lyman. Samuel Insull. C. M. Wilkes.
ATKINSON, WILLARD S.	Electrical Engineer and Factory Manager, Zindars & Hunt; R. F. D. No. 5, New Brunswick, N. J.	W. C. Andrews. W. D. Weaver. T. C. Martin.
BAMBER, WILLIAM CHILDS	Electrical Engineer, Rapid Transit Subway Construction Co.; residence, 330 W. 57th St., New York City.	L. B. Stillwell. W. B. Potter. T. D. Boyles.
BENNETT, WILLARD S.	The S. S. White Dental Mfg. Co., New York City; residence, Charles Place, Elmhurst, N. Y.	H. V. A. Parsell, Jr. George H. Guy. T. J. Smith.
BROWN ROBERT G.	Electrician, 158 Montague St., Brooklyn, N. Y.	T. D. Lockwood. P. B. Delany. A. A. Knudson.

BUNKER, ARTHUR CLIFFORD	Engineering Department, Stanley Electric Mfg. Co., 238 Crocker Building, San Francisco; residence, 1604 Central Ave., Alameda, Cal.	J. F. Kelly. C. C. Chesney. D. B. Rushmore.
RLTON, WILLARD GILBERT	Assistant to Chief Operating Engineer, Chicago Edison Co., 139 Adams St., Chicago, Ill.	James Lyman. Samuel Insull. C. M. Wilkes.
CARPENTER, HENRY CANNON	Assistant in Engineering Department, New York Telephone Co; residence, 113 East 69th St., New York City.	F. B. Crocker. G. F. Sever. R. W. Pope.
CONKLIN, OLIVER FRANCIS	Consulting Electrical Engineer, The Robbins & Myers Co., Springfield, Ohio.	G. F. Sever. H. H. Rustin. B. J. Arnold.
COX, FRANK POWELL	Electrical Engineer, General Electric Co., Lynn, Mass.	Elihu Thomson. E W. Rice. C. P. Steinmetz.
FERGUSON, LOUIS ALOYSIUS	General Superintendent, Chicago Edison Co., 139 Adams St., Chicago, Ill.; residence, 1401 Davis St., Evanston, Ill.	James Lyman. Samuel Insull. C. M. Wilkes.
GEAR, HARRY BARNES	General Inspector, Chicago Edison Co., 139 Adams St., Chicago, Ill.	E. O. Schweitzer. James Lyman. Samuel Insull.
HARRIS, GRENVILLE A.	Electrical and Mechanical Engineer, Takata & Co., 10 Wall St, New York City; residence, Elizabeth, N. J.	W. K. Dunlap. C. F. Scott. F. B. H. Paine.
HEANY, JOHN ALLEN	Expert, Peter Heany Development Co., 904 Quarry St., Philadelphia, Pa.	T. C. Martin. Max Osterberg. Max Loewenthal.
JANISCH, CHARLES	Chief Engineer, The Siemens & Halske Co.; Siegmunds No. 12, Berlin, Germany.	A. H. Kruesi. C. P. Steinmetz. W. I. Slichter.
JUNKERSFELD, PETER	Assistant to Mechanical Engineer, Chicago Edison Co., 139 Adams St., Chicago, Ill.	James Lyman. Samuel Insull. C. M. Wilkes.
KRUESI, PAUL JOHN	Assistant to Sales Manager, General Incandescent Arc Light Co., 572 First Ave.; residence, 330 West 57th St., New York City.	Samuel Insull. C. P. Steinmetz. Charles Batchelor.
LUKES, GEORGE HOLT	Night Operating Superintendent, Chicago Edison Co., 139 Adams St., Chicago, Ill.	James Lyman. Samuel Insull. C. M. Wilkes.
MORGAN, GODFREY	General Manager, The Niagara Gorge R. R. Co.; residence, 815 Niagara Ave., Niagara Falls, N. Y.	F. A. Pattison. H. B. Coho. H. E. Heath.
NIESZ, HOMER ELDERDGE	Assistant to General Superintendent, Chicago Edison Co., 139 Adams St.; residence, 4211 Ellis Ave., Chicago, Ill.	James Lyman. Samuel Insull. C. M. Wilkes.

PALMER, D. ALONSO	Superintendent, Campania Anonima de Redes Telefonicas de Ponce; Box 135, Ponce, Porto Rico.	C. C. Haskins. C. H. Wilson. R. W. Pope.
PALMER, GRANVILLE ERNST	Salesman, H. C. Roberts Electrical Supply Co., 802 Equitable Building, Baltimore, Md.	G. A. Thomson. J. J. Bellman. Horace Dowie.
SMITH, SAMUEL NEWTON	Manager of Sales, Submerged Electric Motor Co., 721 Temple Court, Minneapolis, Minn.	G. F. Sever. Luther Stieringer. P. B. Woodworth.
SWEETLAND, RALPH	Electrical Inspector, New England Insurance Exchange, 55 Kilby St., Boston; residence, Natick, Mass.	H. G. Reist. C. M. Goddard. F. E Cabot.

Total 26.

Transferred from Associate to full Membership:

CARL L. DE MURALT, Baden, Switzerland.

THE PRESIDENT:—Gentlemen, members of the INSTITUTE and guests, the topic of to-night's discussion is the speed regulation of alternators, or rather the speed regulation of prime movers, in its relation to the parallel operation of alternators. We have a number of papers, the principal one by Mr. Keilholtz, on Angular Variation in Steam Engines, and several shorter papers from a number of gentlemen familiar with this subject, who will give us their experience. Consequently we have strictly to limit the time, and it has been decided to permit fifteen minutes for the abstracting of the main paper, ten minutes for each successive paper and five minutes for discussion. The various papers will now be presented.

[Papers by Messrs. Keilholtz, Slichter and Berg have already appeared in issue of October, 1901. Other papers at this meeting by Messrs. Steinmetz and Emmet follow.]

A paper read at the 137th Meeting of the American Institute of Electrical Engineers, New York, October 25th, 1901, President Steinmetz in the Chair.

SPEED REGULATION OF PRIME MOVERS AND PARALLEL OPERATION OF ALTERNATORS.

BY CHARLES P. STEINMETZ.

Regarding the effect of speed regulation of prime movers on the parallel operation of alternators therefrom, three features have to be considered.

1. The permanent variation of speed, due to a change of load.

2. The temporary change of speed, due to a sudden change of load.

3. The periodic change of speed during each revolution.

1. With a change of load on the alternator, the power supplied to the prime mover must be changed correspondingly. This is done by a regulating mechanism, which is is based either on the speed (speed governor) or the acceleration (inertia governor). The speed of the prime mover is a function of the load, usually decreasing more or less with the increase of load. Since, however, alternators in parallel operation, and thus the prime movers driving them, must run in synchronism with each other, it follows that the division of load between alternators driven by independent prime movers depends on the division of power between the prime movers at (electrical) inequality of speed, but not upon the characteristics of the alternator, thus it cannot be changed by a change of excitation of the alternator, etc.; but only by reacting upon the governor (or by a change of belt tension with alternators belted to the same shaft). Therefore for parallel operation of alternators a certain drop in speed with increase of load is necessary, and with a governing mechanism maintaining absolutely constant speed at all loads, the division of load becomes indefinite and parallel operation ceases to be feasible.

If the drop of speed with increase of load is not uniform, but the speed almost constant for moderate load, alternators may operate satisfactorily in parallel at load, but see-saw at light load.

2. With a sudden change of load, since time is required for the governor to act and for the flow of power to the prime mover to change to the condition corresponding to the changed load, a temporary change of speed occurs, larger than corresponds to the change of load, and to reduce this variation of speed, storage of energy by the momentum of the fly-wheel is necessary. With the steam engine the momentum of the steam, as expansive fluid, is usually negligible in its effect, and the temporary effect of speed depends mainly upon the rapidity of the action of the governor. With water power, frequently the momentum of the moving mass of water predominates as a cause of the speed fluctuation with sudden change of load, and the speed fluctuation thus depends upon the momentum of the total moving mass of water. Stand pipes, relief valves, and deflecting nozzles represent means to reduce or eliminate the effect of the momentum of the moving water column upon the regulation of speed at sudden change of load.

3. Most rotary prime movers, as water-wheel, steam turbine, etc., give a uniform supply of power, and thus uniform speed during the revolution. Others, however, as reciprocating prime movers, steam and gas engines, supply the energy by a number of impulses, and the torque, and therefore speed of the prime mover thus periodically varies during the revolution. The resultant torque of these prime movers consists of the pulsating torque of the energy supply (indicator diagram), the alternating torque of acceleration and retardation of the reciprocating masses, the alternating torque due to the finite length of the connecting rod, with vertical engines, the alternating torque of gravity in the ascent and descent of the reciprocating masses, and in gas engines also the pulsating negative torque of the compressor. The speed variation resulting herefrom can be reduced by such a design of the prime mover, that the different components of torque superimpose upon each other to a more nearly uniform torque, or by the use of a heavy fly-wheel. The most objectionable result of this periodic variation of speed is, however, the hunting of alternators and synchronous apparatus (as motors and converters), and this tendency to hunting seems to be aggravated by the use of heavy fly-wheels on the prime mover.

Electrical motors as a rule are not considered as prime movers, although no reason appears for making a distinction between the supply of power through a hydraulic pipe-line or through an electric transmission line. With electric motors the rate of rotation is uniform and the periodic variation of speed thus absent, and due to the practical absence of a time lag, the temporary variation of speed at a sudden change of load absent also, constancy of the supply voltage assumed. With continuous current shunt motors a cumulative series field may be necessary for parallel operations of alternators to secure the necessary drop of speed with increase of load. With induction motors, while the drop of speed under load may be considerably less than required for good parallel operation of engine-driven alternators, the speed variation with the load is so uniform that no difficulties are met.

In parallel operation of alternators driven by synchronous motors due to the absolute constancy of speed under load, an entirely different set of phenomena occurs, similar in some respects to the parallel operation of rigidly connected alternators, but differing therefrom, due to the flexibility of the synchronous motors in their relative phases, but rigidity in frequency.

Difficulties with parallel operation of alternators driven from separate prime movers may be due to:

1. *Lack of synchronizing power*, due to excessive armature reaction. This is practically unknown with modern alternators, in which the synchronizing power is always far in excess of the requirements of keeping them in step.

2. *Surging or hunting* between the alternators. Regarding this, forced surging and cumulative or resonating surging may be distinguished. With prime movors giving a periodic variation of speed during the revolution, periodically varying cross currents flow between the alternators and between alternators and synchronous motors, in extreme cases even between alternator and induction motor, of an amplitude depending upon the amplitude of the speed variation. Under certain conditions this surging becomes cumulative, gradually increasing in amplitude so that the machines may break out of synchronism. The cause of this cumulative effect may be found in the electrical circuit in certain relations between the momentum of the moving mass and the electric constants of the circuit, or it can be found in the mechanical construction of the prime movers, fore-

most their governors. When due to the former cause it can occur also with turbine-driven prime movers and synchronous motors; while the mechanical hunting of the governing device which is the more frequent and serious phenomenon, is most frequently found in direct-connected, engine-driven plants.

The cumulative hunting is overcome by eliminating its cause; that is, breaking the resonance condition of the electric circuit by interference, or by changing its constants and damping the governor so as to stop its ability to hunt, or by reducing the amplitude of the hunting by a damping device in the electric circuit, usually in the field of alternators or other synchronous apparatus.

A paper read at the 157th Meeting of the American Institute of Electrical Engineers, New York, October 25th, 1901, President Steinmetz in the Chair.

PARALLEL OPERATION OF ENGINE-DRIVEN ALTERNATORS.

BY W. L. R. EMMET.

There has been and is much diversity of opinion and practice among engineers in relation to the operating of alternators in parallel from steam engines. This paper explains a method of overcoming the difficulties of such operation which has been in successful use for three years in many important installations.

The problem to which it relates is one of the greatest importance, and the solution described is, or is believed to be, complete and universal. It is, for every reason, desirable to the whole fraternity of electrical engineers, that differences of opinion and of method in such a matter be removed or reconciled. I hope, therefore, that this paper, with the discussions which may follow it, will serve either to establish generally the use of the methods proposed, or to bring about the general acceptance of other or improved means for accomplishing the same result.

The problem of operating engine-driven alternators in parallel arose in Europe before it attracted attention in this country. Our early alternating experience was, for well understood reasons, confined to small generators operated on separate circuits. The use of direct-coupled engine-driven alternators did not begin in this country until about 1893, and the cases of parallel operation were quite infrequent until three or four years later. During the years of '95, '96 and '97, I was concerned in the installation of a number of machines of this type which were operated in parallel, but, for various reasons, none of the troubles characteristic to the parallel operation of alternators occurred in these

cases, so that up to the year 1898 I had not encountered the problem here discussed and had done nothing towards its solution. In Europe the problem had arisen much earlier, and serious troubles had been encountered which were described in the different electrical journals. These accounts of trouble, combined with our knowledge of phenomena of hunting in synchronous apparatus, led me to be apprehensive about the parallel operation of alternators under certain conditions, before the difficulty was actually encountered.

The first case of trouble which I encountered was in the fall of 1898, when two 800 k. w. 60-cycle alternators operated at 100 r. p. m. from McIntosh and Seymour engines were put into operation in Cleveland. These machines, when thrown into parallel, immediately began to surge so violently as to render service impossible. About the same time we started two 1000 k. w. 60-cycle machines of the same type in Philadelphia and four 1500 k. w. 60-cycle machines at 120 r. p. m. in Boston. In both cases the same trouble appeared that had occurred in Cleveland, except that it was worse in both the latter cases, particularly in the case of Boston, where parallel operation was at first impossible under any condition of load.

I happened to be responsible for the engineering of these three plants, and found myself confronted with a very serious problem. I carefully considered the available European information which bore upon the subject, and found that two remedies for this trouble had been there applied. One of these remedies was described in an article by Mr. Gisbert Kapp which came to my atttention. This article explained that the period of oscillation in relative motion of parallel alternators was govered by two quantities; namely, the electrical synchronizing force, and the weights of the moving systems. Mr. Kapp explained that these quantities could be estimated and compared, and an arrangement could be adopted by which the natural period of oscillation between machines would not conform to the strokes of engine, or would be of such a frequency that trouble was unlikely to result. The other method of cure I found described by M. LeBlanc, who advocated the use of deadeners in the form of windings or attachments to the field structure in which currents would be generated by the shiftings of flux, and which would so serve to consume energy and interpose an electrical friction which must tend to kill the oscillations.

A careful consideration of both these remedies indicated to my mind that neither of them afforded an adequate solution of the difficulty with which we were confronted Special design of dynamos to obtain certain synchronizing effects with certain loads was almost out of the question, since commercial dynamos should be adapted to operation under all conditions, so that the investment in them may not be lost when change of condition is brought about through growth or other circumstance. M. Le-Blanc's method was also objectionable, since it introduced devices which must occupy valuable space, and complicate the design of machines, and which must, under operating conditions introduce more or less serious losses. It was furthermore reasoned that a simple deadener could not be complete in its effectiveness since the deadening effect must increase in proportion to the oscillation, and must be very small where the angle of oscillation is slight. It therefore seemed that this disappearing quantity would probably limit the extent of oscillations, but could hardly be expected to entirely prevent them under all conditions. Our subsequent experience showed that both of these methods were sound in theory and practicable within certain limits, but also developed the fact that they could not be depended upon as a basis of commercial work under various conditions.

At the same time that these theories were being considered I started a set of investigations with a view to ascertaining the exact conditions which existed in engine and dynamo while these troubles were occurring. In conducting these investigations, I reasoned that there must be a powerful actuating force employed in keeping these oscillations alive, since the waste of energy incident to their maintenance must necessarily be large, even where no pole-piece attachments or other form of deadeners are used. Theory clearly indicated that the oscillation was a pendulum action, whose period was governed by the weight and by the synchronizing power. I reasoned that this pendulum continued to swing in spite of a heavy friction, and I conducted my investigation with a view to ascertaining the actuating force. I presumed that this actuating force must come from the steam in the engine cylinders, and I had careful sets of indicator cards made showing the impulses under successive strokes and in the different cylinders. The comparison of these indicator cards immediately showed that there was a large irregularity in the delivery of steam to engines, and that the differences of area in

different cards or in cards which should be the same under fixed load were very great. It was further observed that these variations arose from periodic motions of the governor, and that they conformed in period with the swing of the machines, and with the fluctuations of electro-motive force and current.

These facts, which were communicated to me by letter from Cleveland, led me to believe that the cure for the trouble must be sought in a prevention of a rapid periodic motion in the engine governors. I explained this in letters to the engine builders, suggesting to them that retarding dash-pots be used on their governors, which would prevent these sudden motions. The builders of the Cleveland engines, in return, replied that their governors were fitted with dash-pots, and that they were of the best anti-hunting type then known to the art. By such correspondence I soon discovered that the matter was entirely beyond the practical experience of the engine builders, and that we could hardly expect them to afford us a solution of the problem. I then went myself to Philadelphia, where the conditions for experiment were favorable, first communicating my theory in writing to the purchaser and to the engine builder as a matter of record. The governors of engines in Philadelphia were so arranged that dash-pots could be very readily attached to them, which would limit the speed of their motion to any desired degree. Our first experiment was to block the governors positively and run with fixed load. Under this condition, no sign of oscillation appeared. We then operated the engines under various conditions of load and excitation, with and without dash-pots having various degrees of stiffness. These investigations showed that the tendency to oscillation gradually disappeared as the freedom of the governor was limited by the dash-pot. Under certain conditions a small dash-pot effect would kill the oscillation altogether, while under other conditions a stiffer dash-pot would be found necessary. In every case, however, the dash-pot effected improvement, and the cure seemed to be simply a matter of degree. In this particular case, it was found that satisfactory parallel operation under all conditions of service could be obtained with dash-pots which were not so stiff as to materially interfere with the quick action of the engines, and such a cure was adopted, so that the investigations were not carried further in this plant.

From my experience in Philadelphia, however, I reasoned that other cases would arise where such a cure could not be applied,

since the dash-pots necessary to afford stability of parallel operation would be too stiff to admit of the proper quick governing of the engines. These anticipations were very soon realized in the operation of the machines above mentioned at Boston. In these machines the oscillating tendency was so strong that they could not keep in step at half load with dash-pots so stiff as to admit dangerous variations of speed with sudden load fluctuations From this experience it was seen that we must find some means of deadening the motions of the governor occasioned by the oscillations, and at the same time we must leave the governor free to move where actual changes of load came upon the machine. A means of accomplishing this result was devised in Cleveland by the combined efforts of Mr. H. W. Buck, my assistant, and Mr. Harte Cook of the McIntosh and Seymour Co. The device is shown in Fig. 1, which shows only one of many forms devised for accomplishing practically the same result. The main piston rod shown in this sketch is connected to the governor weights in such a manner as to be moved directly by them. The space on both sides of the piston and all the port spaces are filled with a heavy grease. By-passes, F, are provided, through which this grease can flow from one side of the piston to the other. Each of these by-passes is actuated by a small piston-valve, C, backed by a spring, G, and provided with a small by-pass, B. The practical effect of this device is that the springs tend always to keep the gates of the main by-passes closed, and that the piston is thus virtually locked until the pressure at one end of it has forced the by-pass valve back. The first motion of the main piston is thus limited by the size of the small by-pass, B, whereas its later motion, after the valve has been forced back, is limited only by the size of the main by-pass, F. It will be readily seen that this dash-pot can be so adjusted that it will act as a temporary lock on the governor mechanism, and that it will release entirely after the tendency to govern has continued for a certain length of time.

Such dash-pots were first applied to the engines above mentioned operated in the plant of the Boston Electric Light Co., and it was found that they afforded a positive and complete cure of the oscillating troubles which had been experienced there. By their use, with proper adjustment, the engines can be operated under all conditions of load, and with any number in parallel, without the slightest indication of the oscillating trouble which

was at first so difficult to prevent. In this case the tendency to oscillation was much more positive than in any other which has come to my notice, and the perfect success of the remedy has led me to believe that the method is universally applicable, and that all similar troubles can be cured by its intelligent application.

In order that this form of dash-pot may properly accomplish its functions in the most difficult cases, it must be made strong enough mechanically and hydrostatically to bear, for a time, the entire force imposed by the governor weights in their effort to assume a new position, and the period of such opposition must be such that the dash-pot will not unlock within the time of a single period of oscillation when the machines are operating in parallel. It is also necessary that the dash-pot be kept full of grease, so that there will be no lost motion through air spaces. If the dash-pot falls short in any of these particulars, the remedy which it affords will only be partial, and in difficult cases it may prove inadequate. It is only in certain difficult cases that we need use such complete preventives as were found necessary in Boston.

While discussing this subject, it may be profitable to briefly touch upon a few other facts which have developed in the course of my experience in the parallel operation of alternators. It is popularly supposed that it is necessary to use very heavy fly-wheels in order that alternators may be successfully operated in parallel; it being the custom of some engineers to consider a guarantee of small angular variation as the equivalent to a guarantee for parallel operation. My experience has rather indicated that generators with light fly-wheels are most easy to operate in parallel. The frequency of natural oscillations in such machines is high, and the conditions of engine operation are generally unfavorable to their development. Large fly-wheels are desirable on direct coupled alternators, because a steady frequency is a valuable feature in any system. The requirements of parallel operation, however, are rather unfavorable than otherwise to the use of heavy fly-wheels.

The peculiar periodic action of an engine's governor which I have before described, is not confined entirely to the parallel operation of alternators, but also may occur and cause trouble where a single alternator is operating a load of synchronous apparatus. We may even have alternators which will operate together in parallel, but which will, through periodic action of the governors, give trouble with certain loads of synchronous

apparatus. It is thus desirable in all cases to prevent this periodic action of the governors, and by this prevention many troublesome difficulties may be lessened or overcome.

The behavior of all parts of the system while this oscillating trouble occurred in parallel operation was very beautifully illustrated during our experiments in Philadelphia. The shafts of engines were in line, and the relative positions of poles could, at all times, be seen, the poles of one machine appearing to stand still when the machines were in perfect synchronism. The movements of the governor mechanism could also be seen by oscillations of a lever arm which was in the line of vision. Ammeters and voltmeters were also placed in the same line. It was possible at a glance to see the degree of angular displacement of the poles, the degree of motion of the governor, and the fluctuations of the current and voltage. An observation of these conditions showed very clearly the exact character of this phenomenon. It was found that under some conditions the oscillation was an extremely sensitive matter, and had very little power of sustaining itself. A slight change in the field strength of one of the machines would sometimes break it up so that it entirely disappeared, after which the machines might remain steady for some time and then gradually begin to swing again. Under other conditions it was more persistent. Everything indicated, however, that it could not exist for any appreciable period without the actuating force afforded by the irregularity of steam impulses.

Discussion.

The President:—Gentlemen, we have listened to a series of very interesting papers. I desire to draw your attention to one point in Mr. Slichter's paper—the figure which he mentions toward the end, less than one degree displacement. This is very interesting considering that the engine has by no means an abnormally heavy fly-wheel. Very often you find steam-engine builders, if you specify two-and-a-half or three degrees displacement, claim that engines of so close regulation cannot be built without enormous fly-wheels. But here you see, apparently without great difficulty, a value reached very much below such specification. There are a number of gentlemen here who can speak on the subject. I am sorry they could not send us the manuscript beforehand, so that it could be distributed to you some days before the discussion. There is a communication from Mr. R. H. Rice which I will call upon the Secretary to read.

The Secretary read the following

Communication from Mr. R. H. Rice.

In designing engines to operate alternating current generators in parallel, two features of the design must be carefully worked out. These are:

I. The fly-wheel.

II. The governor.

I. In determining the proper fly-wheel effect for any given case it is necessary to know the variation in polar distance allowable from mean or uniform rotative position in either direction. This information must be furnished by the designer of the generator. It is also necessary to know the form of indicator card expected to be realized, and the weights of the reciprocating parts of the engine, also for exact determination, the moment of inertia and center of gravity of the connecting rod Mr. Keilholtz has shown the method of obtaining curves showing the velocity of any point on the fly-wheel at any position of the stroke, also any point in reference to mean position or that due to uniform rotation.

The writer believes, however, that graphic methods can be used to much greater extent in the determination of these curves, and had hoped to be able to submit diagrams showing the methods employed. The time allowed, however, has been insufficient to do this. The graphic methods give much greater clearness of perception in estimating the value of the various modifications of design; for instance, the determination of which crank shall lead, the high or low pressure; and in difficult cases it is readily determinable whether some other crank angle than 90° will prove sufficiently beneficial in reducing disturbances to make it advisable to adopt this arrangement.

It is usually the custom to deduce curves showing velocity variation and space variation for half, and full load and 50% overload, using, in determination of fly-wheel weight that one of the three space variation curves which gives the maximum result. In determining this curve of space variation, the mass of rotating parts can be conveniently taken as unity at the crank pin, and by simple proportion the maximum variations found can be reduced to the required amounts by the addition of mass at this point. It is then easy to reduce the mass at the crank pin to an equivalent mass at the fly-wheel rim, and thus the fly-wheel weight is adjusted to the requirements of the generator.

In practice, variations in steam distribution and other accidental causes introduce irregularities not allowed for in preceding calculations, to meet which an addition or safety factor of 30% to 50% is used in determining the weight of fly-wheel.

II. The type of governor used is important, because

(*a*) It must be readily adjustable in characteristics to meet the varying requirements of different voltages and frequencies, and to provide for the closest regulation consistent with proper division of loading between two or more machines.

(*b*) Two or more governors must be readily brought into synchronism as regards rate of decrease of speed with increase of load.

(*c*) The governor must be readily adjustable as regards damping or dash-pot effect.

(*d*) It must have sufficient power to retain control of the speed even under considerable dash-pot resistance.

(*e*) It must be adjustable for speed from the switchboard, to provide, first, for equalizing the speed of an unloaded unit with those carrying the load, in order to get the units into step; second, to make the unloaded unit assume its share of the load after putting it into step; third, to permit of relieving a unit from load so that it may be put out of service.

As concerns the low or medium speed engine with automatic trip cut-off, the inertia governor, belted or geared to the engine shaft, seems to possess all the above characteristics in a marked degree, its great power and rapidity of action, even under considerable dash-pot resistance, permitting of reducing the fly-wheel to dimensions sufficient to meet the requirements of the generator as previously explained, no addition of weight to the fly-wheel being necessary to give time for the governor to act.

The lighter the fly-wheel, the less the intensity of the pendulum effect, described by Mr. Emmet, and the less the resistance necessary to apply to the governor in the shape of dash-pot resistance. Dash-pots which release the governor, in case of tendency to motion of the governor weights, continuing for an appreciable time, as described by him, are undoubtedly the most generally effective.

The apparatus most satisfactory for varying the speed char-

acteristics of a governor consists of a simple reversing motor of the d. c. type, which screws up or down a spring attached to the governor weights, the motor being started, stopped or reversed from the switchboard by a simple switch. By this means the switchboard attendant is placed in entire control of the situation.

Of two-crank engines, the twin engine, non-compound, operates in parallel most easily. The cross-compound engine, by reason of the impossibility of accurately dividing the loads between the two cylinders, for wide ranges of load, is more difficult to operate. This is, however, now the favorite type and can be used without fear of necessity for excessive fly-wheels. The double tandem engine does not give the good results which it would seem to possess, on account of its greater complication. Vertical engines require larger fly-wheels than horizontals, as the weight of the reciprocating parts acting vertically downwards introduces an irregularity in the rotative forces.

Three cranks, theoretically, give a great increase of steadiness, which is diminished in practice by difficulties of steam distribution. All in all, economy of steam being considered, the horizontal cross-compound seems the best all-round type, with the vertical form second.

THE PRESIDENT:—Gentlemen, we would like to hear now from Mr. Seymour, of McIntosh, Seymour & Co.

MR. JAMES A. SEYMOUR:—When requested by Mr. Mailloux to make some remarks on our experience with engines suitable for operating alternators in parallel, it occurred to me that while this subject has come to be quite generally understood by engineers having a special interest in it as evinced by the thorough and exhaustive papers which have been presented this evening, there often seems to be a lack of accurate knowledge of the requirements necessary to successful operation among those who may be contemplating the purchase of such apparatus; and, together with the generator and engine salesmen, they quite often discuss the problem from mistaken view-points or with a feeling of uncertainty, notwithstanding the correct expositions of the subject which have been published. Therefore, it seems to me that in order to clear up the subject, some discussion of these popular misapprehensions is necessary.

One notion, not so common now as formerly, is that close regulation is the principal quality desired. The ordinarily accepted and proper meaning of the term "close regulation" relates only to the difference in the mean speed of an engine between full load and no load as affected by the control of the steam supply by the governor. The requirements in this line for this kind of service are not greater than for any other; in fact, some of the builders of alternating current machines are now recommending a somewhat more sluggish regulation than has been usual in direct current work in order to simplify the problem of dividing up the load properly between the parallel running units. Mr. Stein-

metz clearly points out that with no change in speed the division of the load is indeterminate.

The difference in the requirements for the proper running of a single unit from the requirements necessary to run two such units in parallel is not generally understood. In the first case, in order to avoid trouble with subsidiary apparatus, there must be a certain approximation to uniform motion throughout the revolution. This is secured by increasing the balancing effect of the fly-wheel to the proper amount, which may be determined by the very complete method given in Mr. Keilholtz's paper. When two such units are thrown in parallel, the principal difficulty to be avoided is the hunting or surging of the generators. Some of the causes of this trouble seem to be less understood than anything else connected with the subject. As pointed out by Mr. Emmet, the heavy fly-wheels which are necessary for proper uniformity of rotation, as mentioned above, increase the chance of trouble from the hunting of generators. This is quite contrary to a popular idea that sufficient uniformity of rotation insures correct operation in parallel.

Of course, in one sense, it is true, that if two units run in parallel with the revolving parts of both, always within a certain deviation from the position of absolutely uniform motion, there can be no harmful surging; but as we use the term "deviation from the position of uniform motion," it refers only to the deviation of a unit running singly at some constant load between no load and full load, such deviation being due to the irregularity of the torque of the applied forces, and being limited by the balancing effect of the fly-wheel. The trouble is when two such units are put in parallel, the deviation may be found to have become much greater. This increased deviation is the "cumulative" deviation referred to by Mr. Steinmetz, and may be caused by the combined action of the governors and the "pendulum-like" oscillations described by Mr. Emmet. Since the effect of this oscillatory movement is to simultaneously retard the speed of one unit and quicken that of the other, the governor of the unit which is being retarded will act and will cause more steam to be admitted; while in the other unit the steam supply will be similarly diminished. It will be noted that the governors, in doing this, are only performing their normal functions as speed regulators. There is necessarily an interval of time between the commencement of governor action and the change of speed resulting from the effect of such action on the steam supply. This I will call the time interval of governor action. A corresponding time interval for the swing or half period of the pendulum-like oscillations of the governors will result in a reversal of the swing of each unit by the time the change of speed is effected by the governor, and therefore, in an impetus to the faster unit and a retardation of the slower unit. This will cause a building up of oscillations until parallel running is impossible. If the time

intervals correspond, as above, the oscillations will be built up from even a very small displacement of phase, which may result from the machines not being initially thrown into exact phase, or from the allowable deviation from uniform motion of the engines, or from other causes.

The cause of surging is quite generally described as due to the hunting tendency of the engine governors. This is often not correct and gives a wrong impression as to the nature of the trouble. It is true that if the governors are over-sensitive and unstable, they will have a hunting tendency, which will cause surging even with direct-current apparatus. Surging of alternators may also occur with types of valve gear where the cut-off mechanism is so sensitive that slight periodic changes in its position easily affect the cut-off. This may be due simply to lost motion, to the effect of the steam valves on the governor, or to other causes. If the vibration period of the governor weights happens to correspond with the oscillation period of the generators, there will doubtless be an increased tendency to hunt. However, in some of the cases cited by Mr. Emmet where this trouble was first encountered, the surging persisted until the damping of the governor action in both engines was carried to a degree which nearly rendered the governor inoperative. Damping the governor of one unit did not prevent trouble with the other. If the surging had been caused by a harmonic vibration of the governor mechanism, it would seem that a lesser damping, or a damping of only one governor, would have broken it up. It was found necessary in one instance, however, to damp both governors with an ordinary dash-pot to such an extent, that, while the governor would regulate within 2% from full load to no load, when full load was suddenly thrown off, the momentary variation in speed was about 15%. The period of oscillation was observed to be about four revolutions, or two seconds. Any quick acting and sensitive governor will be sensibly affected by a change of speed lasting for half such a period, and the time interval of governor action must be just about right to cause a building up of the oscillation.

Mr. Emmet quotes Mr. Kapp as explaining that the period of oscillation of the revolving part of such units is a function of the synchronizing power and of the inertia of the revolving mass. What I wish to make clear is that where this period is long enough to permit of sensible action by the governor, and where the time interval of governor action corresponds to a half period, as before described, cumulative surging may necessarily occur, even with a perfectly stable governor, which cannot properly be said to have any hunting tendency. Periods long enough to cause this trouble are usually found only with lower speeds and heavy wheels, but these are necessary features for the very large units which are often called for by the kind of service to which alternating work is peculiarly adapted. An obviously sure

remedy is to lengthen the time interval of governor action sufficiently to break up its cumulative effect on the oscillations.

Of course a governor is equally responsive to changes of speed, whether due to changes of load or to the oscillations referred to. As stated by Mr. Emmet, the impossibility of sufficiently delaying the action of the governor by the usual damping devices without ruining it as a speed regulator, led to the invention by Mr. Cooke of the time-delay dash-pot described by Mr. Emmet. This device allows the governor to respond to the least change of speed if persistent beyond the period of oscillation, but gives a heavy damping effect for a shorter period. With this device, it has been found necessary to lengthen the time interval of governor action to such a small extent, that the efficiency of the governor as a speed regulator is not appreciably lessened. U. S. patent No. 677,064 has been granted to Mr. Cooke covering this form of dash-pot. This fact should be published in the TRANSACTIONS of the INSTITUTE simultaneously with Mr. Emmet's paper describing this device; otherwise, the natural inference would be that it is public property. This would be unfair, both to any one who might wish to make use of it and to Mr. Cooke.

Mr. Keilholtz's paper gives a carefully worked-out method of determining the relation between the balancing effect of the fly-wheel and the deviation from the position of absolutely uniform motion. He also gives an interesting experimental determination from an actual case. Owing to the very small deviation obtained with the proper weight of fly-wheel for actual service, and to the very short time intervals, all of which introduce great difficulties in making correct observations, I think Mr. Keilholtz is to be congratulated that his ingenious methods resulted in such a very fair agreement between the measured and the calculated results.

In determining the deviation of engines designed for this service, we use the ordinary and very convenient approximate method described by Prof. Unwin and other writers, and employ a graphical solution throughout. This approximate method assumes the entire weight of reciprocating parts to be concentrated at the crosshead pin. I have determined the difference in results obtained by this approximate method and by the exact method given in Mr. Keilholtz's paper, starting with the turning moments given in the table of his paper, but substituting for inertia moments those obtained by the approximate method. The maximum variation, which is the sum of the maximum deviations ahead of and behind the mean position of uniform motion, is but 42/100 of one per cent. less by the approximate than by the exact method. I do not consider, however, that it would be safe to assume that the error would always be so insignificant.

Mr. Slichter in his paper discusses some formulas, based on the number of steam impulses, for determining approximately the deviation from the position of uniform motion, and rightly

questions the accuracy of this method because it entirely neglects inertia effects. The period of steam impulses in an engine having two cranks 90° apart is one-fourth of the revolution, while the period of inertia, statical, and connecting-rod-angularity effects is one revolution. With a given acceleration the displacement varies as the square of the time. In this case, therefore, the acceleration due to the inertia forces may in one sense be said to be sixteen times as effective as the acceleration due to the pressure of the steam. Even in cases where the piston speed is low, and the forces due to steam pressure are of very much greater magnitude than the inertia forces, the effect of the latter on the deviation will be considerable, and will increase with a rise in piston speed. Also, since, as Mr. Slichter points out, these different forces, when combined, generally tend to neutralize each other, thus making the crank box effort more uniform, a small variation in the relative magnitude of these forces may produce a much greater proportional change in the amount of deviation.

In short, it does not seem to be possible to determine, even approximately, the deviation of any engine without combining the torque curves of the different forces and finding by successive integrations the velocity and displacement curves.

It is possible to make the determinations for each crank separately. In combining them the effect of different crank angles can easily be investigated. I have in mind a large vertical cross-compound engine which has nothing about it out of the ordinary, except a rather high piston speed. The smallest maximum deviation was secured with the cranks 180 degrees apart or in opposite positions. With crank at right angles, one sequence of h. p. and l. p. cranks gave a smaller deviation than the opposite sequence, and with cranks 35° apart, nearly the same results were obtained as with cranks opposite. In this case the effect of steam impulses was so subordinate to the effect of the other forces, that when cranks were arranged to give two steam impulses a revolution, the deviation was smaller than when cranks were spaced to give four steam impulses a revolution.

When a low frequency is used, if the weight of fly-wheel is determined solely by the deviation requirements, it will not be heavy enough in some cases to serve as a proper balance wheel for the engine—having in mind the ordinary requirements for an engine working under variable load in any kind of electric service.

In regard to the method of throwing machines into step, some operating engineers prefer to do this by simply throttling down the light engine. Others prefer a speed-changing device by which the governor can be adjusted while running. Some such device is very convenient for dividing up the load between engines, since it avoids the necessity of keeping the governors always in accurate adjustment.

The first engines which we installed for this service were used

with 300 k. w. generators running at 150 revolutions. Although, as mentioned before, the division of the load is easier with somewhat sluggish regulation, these engines were without difficulty thrown into step with each other by throttling, were run in parallel operation without hunting, and divided up the load properly with a variation from full load to no load of 1½ per cent. There was nothing special about the construction of the governors, no time-delay dash-pots were used, and yet there were no troubles from surging. The wheels were light, in view of the fact that the engines were of the tandem type. This helped out in the matter of surging, as did also the speed; the 60-cycle frequency was against it, however. Considerable attention is necessary to keep them in adjustment so that the load will be properly divided. These engines were installed without any conception of the troubles which have ultimately developed with units of larger size running at lower speeds. It would probably not be safe to count on duplicating this result.

Some time since, in connection with the installation of three Westinghouse two-phase, revolving armature, 500-volt, 60-cycle, 1,500 k. w. units running at 116 r. p. m., and one smaller unit of 750 k. w. capacity, after the contract for the engines was closed, we received a request from the engineering department of the generator contractors, asking for a fall in speed between no load and full load of 5%, for the purpose of aiding in proper divisions of the load. When the engines were started up, the consulting engineer for the company which was installing the plant stated that he had contracted for engines to regulate within 2%, and objected to 5%. Therefore, the engines were adjusted to give close regulation, in accordance with his view. These engines were provided with time-delay dash-pots, and also an attachment to the shaft governor for changing the speed. This device is operated electrically from the switchboard while the engine is running.

I give in full, reports of two series of tests made by the station operating force with these units. These tests show the performance of the units when running in parallel under different conditions, and indicate no trouble from dividing up the load, notwithstanding the close speed regulation by the governors. The first series is as follows:

First.—Our No. 3 engine was loaded up to about 1,200 k. w., the load being delivered to a water rheostat through the raising transformers. The No. 2 generator was then started up. By the use of the governor motors the speed of No. 2 was easily adjusted so that the phase lamps made one cycle in about one-half minute. The best speed of the phase lamps cycle for synchronizing is about one cycle in two seconds. When this speed was obtained the two generators were thrown in multiple on the high tension side of the raising transformers. About 200 amperes per phase (the generators are 500-volt, 2-phase) showed up on the ammeters.

A slight adjustment of the field rheostat cut this current down to zero. Each generator is equipped with Westinghouse long-scale indicating wattmeters. The total swing of these wattmeters on the two engines was about 100 k. w. on each meter. The probable interchange of energy was not over 25 to 50 k. w. per phase, and made absolutely no effect on the voltmeters, nor was there any change of current which could be seen on the Westinghouse long scale ammeters. The governor motor of No. 2 was then speeded up, and in about one minute the load was balanced between the two machines. The No. 3 governor motor was then run in the "slow" direction while No. 2 motor continued to run in the "fast" direction. After about ½ minute the entire load was on No. 2, and a slight adjustment of the field rheostat, necessary on account of the regulation of the generator, cut the current on No. 3 down to zero. When the two machines had equal loads, the load was changed from as low as 200 k. w. per machine up to 1,000 per machine, and the loads were divided in exactly the same proportion at all points, and during this change of load the oscillations on the wattmeters were not more than 50 k. w. per meter, the time being 75 to 100 oscillations per minute, and did not apparently correspond to the revolutions of the engine. No attempt was made to get the engines in the same crank positions; in fact no observations were taken on this point.

Second.—A large engine and a small engine have been run together with about 100 k. w. total load, and the fluctuations of the wattmeters were about 100 k. w. per phase. These fluctuations were very rapid, perhaps 100 per minute, and rather more severe than in the above case.

Third.—We can run a large engine and a small engine in multiple with a considerable amount of load, and the wattmeter fluctuations are very moderate. The load can be shifted from one to the other without inconvenience, and nearly as easily, if not quite, as in the first case.

In no case do the ammeters show any fluctuations whatever, and the wattmeters, which are very delicate instruments, are the only means we have of telling that there is a fluctuation in the current; in fact, we consider the parallel running of our machines absolutely perfect, and, indeed, much easier than with the average d. c. plant. We have not, however, had any considerable amount of experience in this feature in actual running conditions other than above specified, since our load has been very light.

Following is the second series of tests, made several months later:

A test was made of two of our direct connected generators running in multiple; machines Nos. 3 and 4 were used. The engines and generators are described below. These machines were put in multiple on the high-tension side of raising transformers at 11,000 volts, and under the following conditions no

fluctuations whatever in the ammeters could be seen, and the fluctuations indicated by wattmeters were *not more than 100 k. w. in any case.* All instruments were of the Westinghouse long scale type.

No. 1. Two machines with both phases in multiple at no load.

450 volts.

Current zero on both phases.

No. 2. Connections the same as No. 1.

1,000 amps. lagging current on each phase, caused by intentionally disproportioning the field current.

No. 3. Connections the same as No. 1.

450 volts.

200 amps. on each phase.

100 k. w. positive on each phase of No. 3 machine. 100 k. w. negative on each phase of No. 4 machine, caused by intentionally reducing the speed of one engine and increasing the speed of the other.

No. 4. Machines in multiple on one phase only.

450 volts.

Current 0.

No. 5. Machines in parallel on one phase only.

1,000 amps. caused by intentionally disproportioning the field current.

No. 6. Machines in multiple on one phase only.

450 volts.

200 amps.

100 k. w. positive on No. 3. 100 k. w. negative on No. 4.

No. 7. Machines in multiple on one phase only.

450 volts.

1,200 amps. on No. 4. 800 amps. on No. 3.

600 k. w. on No. 4. 400 k. w. on No. 3, caused by dropping the plate in water rheostat.

No. 8. 450 volts.

No amps. on No. 3. 2,000 amps. on No. 4.

No k. w. on No. 3. 1,000 k. w. on No. 4, caused by intentionally raising the speed of No. 4 and lowering the speed of No. 3, while the plates were in resistance tank, as in No. 7.

No. 9. Same as in No. 8, with generators alternated. Without changing the field rheostats from their position, as in No. 8, but having caused the electrical speed motors to throw the load onto No. 3, taking it all away from No. 4, a current of approximately 100 amps. appeared on No. 4, and a slight adjustment of the field rheostats was necessary to bring this to zero.

No. 10. Machines in multiple on both phases.

450 volts.

Between 0 and 50 amps.

0 to 50 k. w.

One 400 k. w., 500 v. rotary running at Eighth street sub station and resistance tank plates having been removed.

No. 11. Same as No. 10, with two 400 k. w., 500 v. rotaries at Eighth street. Under this condition the rotaries at Eighth street showed no fluctuation whatever in their current, while with the single generator running at light load there was a fluctuation of about 200 amps. in the nature of "pumping."

The d. c. voltmeter on one rotary, with one engine running light, varied about 5 volts in time with the engine stroke. With two engines, one rotary showed a fluctuation on the d. c. voltmeter of less than 1 volt, and 2 rotaries on 2 engines showed no fluctuation of the d. c. voltmeter. Five observers at the switchboard failed to note any fluctuations in the meters when the two 400 k. w. rotaries were started and thrown into synchronism with the two engines, nor could any current be seen nor any change in the wattmeter fluctuation while the rotaries were operated or disconnected.

No. 12. After shutting down No. 4, No. 3 was operated on the water rheostat with 1,000 k. w. of load, the boiler pressure being 125 pounds. The difference in speed between it and No. 2 generator, which was carrying the commercial load, was shown by the synchronizing phase lamps to be about 15 cycles per minute, No. 3 being this much slower than No. 2. The circuit-breakers of both phases were opened under this condition of load, and the phase lamps made 20 cycles and slowed down to about one per minute, thus showing that the engine did not vary from the other a single revolution while having 1,000 k. w. thrown off. This test was repeated three times with identical results.

Substantially the same results are obtained with the General Electric machines of the same capacity at Boston, referred to by Mr. Emmet, and at other installations where these time-delay dash-pots have been used.

Mr. C. F. Scott:—Mr. President, in the consideration of the parallel running of alternators, there is one point which, while it has naturally been evolved in the various papers and discussions this evening, has not been brought out with the force which seems to me desirable. It is the fundamental difficulty in the parallel operation of alternators, which does not exist, for example, in the parallel operation of direct-current machines. It is the very great difference in the amount of power passing through each alternator caused by a slight difference in the angular positions of their armatures. If two direct-current machines are driven in parallel the machines may surge back and forth through a certain angle, but they will each be delivering practically constant power to the circuit. On the other hand, if two alternators running in parallel attempt to surge a little, the one which falls back may lose its whole load, and the one which is forward may take all the load.

It is quite common to consider this problem of alternator running with particular reference to the cross currents, and the impedance factors, and the wattless currents, and the oscillation, and

the fly-wheels, and other things. But the system primarily is a power system; the purpose of the whole plant is to furnish power, and the alternators themselves are simply the instruments for receiving the power of engines and transmitting it to a circuit. For illustration, we may consider two similar power systems, one of which has the electric elements, and the other has not. The electric system, which is shown in Fig. 1 has two similar engines driving alternators which have wires coming down to bus-bars which supply a load. The second system, which is shown in Fig. 2, has two engines like the others, which drive gear wheels and the gear wheels mesh into pinions on a shaft. That shaft by a pulley and belt drives a load. In both cases engines transmit power through some instrumentality to a common load. In both cases the engines must run at the same average velocity. If one of the engines with gears tends to advance a little it simply relieves the other engine of load; it takes the load itself. If there be slight flexibility in the shaft joining the two, a slight variation in angular position is possible, but when there is any variation in angular position the elasticity of the shaft tends

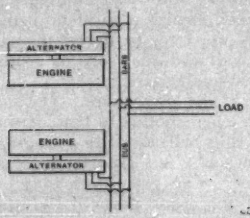


FIG. 1.

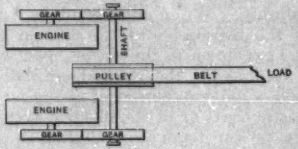


FIG. 2.

to draw back to the common and neutral position. If now, for any reason, one engine does advance a little, it takes more than its share of the load, and a very few degrees displacement will obviously remove all the load from the other engine. In this case the governor of the loaded engine—paying no attention to other things—may immediately proceed to get ready to supply more power; as it has been called upon for more power, it gets ready to deliver more. The other engine being relieved of its load, its governor begins to work the other way, because when less load is on the engine it gets ready to deliver less. The elasticity of the shaft may immediately remove the loads, the engine which lagged may now advance and carry the greater load, see-saw action begins to take place, and if the governors have a little swing of themselves it is easy to see how they can very soon set up an oscillation between the governors, shifting power first from

one engine to the other. The analogy is obvious. The foregoing statements apply also to the electrical system in which alternators must run at absolutely the same mean velocity, but the elasticity, if you choose, of the electric circuit allows some little displacement. Now, if in the alternators we do get this condition, if wattmeters show vibration, and if there are currents flowing, we need not necessarily attribute the trouble to some fault in the alternator. The alternator simply disposes of the power which it receives. It unfortunately does not make any power itself, though it does insist on retaining a little of what it gets, and if wattmeters show variation, that must mean the alternators are receiving varying power from the engines which drive them, which will generally indicate that the supply of power to the engines from the steam is also varying. If two engines will not run alternators in parallel, it is likely that they would not be suited for driving a common shaft by gearing if the "elasticity" and the fly-wheel capacity of the two systems were the same. The problem of parallel running is a mechanical—an engine—problem.

By following out the mechanical illustration of the problem of synchronizing in respect to speed, phase, and other elements, I have often been able to get a very much clearer idea of multiple running. When any problem comes up, I try to divest myself of the intricacies of the flow of current and that sort of thing and get down to the simple mechanical analysis.

I spoke of the mutual oscillation of the governors. If they have a natural period of swing which corresponds to that of the elastic shaft or of the electric system they will assist in swinging the power from one engine to another. The interference of governors will be one method of cure. The governors may be given a period different from that of the other parts of the system by a different adjustment, and in that way they may cease to act in harmony and may cause an interference which will prevent any continued shifting of power. It may be that the governor may come in with an oscillation which will be a positive benefit by tending to break up in the other oscillations.

The matter of fly-wheels must be considered in two different connections. One relates to the angular variation from uniform speed during each revolution. The requirement is that the motion shall be sufficiently uniform so that in ordinary operation of machines there will not be excessive cross currents due to difference of phase between the e. m. f.'s generated by two alternators, as one engine is periodically a little in advance and a little back of the other, due to the irregular motion through the revolution. The fly-wheel must evidently be heavy enough, so that the flow of current shall be within reasonable limits.

On one hand, therefore, the fly-wheel must prevent undue variation being caused by the variation in rotative effort in the engine. On the other hand, it must prevent undue variation

from external causes. The actual oscillation of a fly-wheel of an engine running alone depends on the character of the loads applied. Calculations such as have been given this evening apply to an engine supplying power to a uniform load, a dead load as we call it electrically. If it is supplying a synchronous load, a synchronous motor or a rotary converter, there is chance of hunting, in which the synchronous motor or rotary converter will take power intermittently. In that case there may be a reaction back on the generator, which will cause its angular position to vary from the uniform very much more than it does from a variation of the dead load. In some tests which I directed a year or more ago, this point was brought out quite clearly by a method of measuring the angular position, which was not as complete and elegant as that outlined by Mr. Keilholtz; yet it did very well under the circumstances. A small wheel was made to run at uniform speed. Slots in this wheel gave an intermittent ray of light at regular intervals, which was thrown on the surface of the fly-wheel, on which equally spaced lines were drawn. The conditions were investigated carefully, and it was found, from a little theoretical calculation, just what width the lines should appear if the wheel was running with absolute uniformity. I think the width of the clear white lines on black ground was about a sixteenth of an inch. If, then, the fly-wheel moved uniformly, these lines should all appear in the same place. If, however, it did not move uniformly, but was sometimes a little in advance and sometimes a little behind the mean position, these white lines would show a little above and a little below at different times, and as the speed was quite rapid they would appear in the shape of a band, which would be one-eighth, a quarter, half an inch in width, and by noting the width of this band the total amplitude of the vibration of the fly-wheel was determined. When running empty or carrying a load of incandescent lamps or induction motors, there was a certain slight variation in speed, which could be noted and read quite accurately. When a certain rotary converter was started, the motion continued about the same for a little while and then increased in amplitude, and soon ran out to quite violent proportions, showing the reaction of the rotary converter on the engine. This engine had a fly-wheel which was considerably lighter than another engine which was running other rotary converters of the same kind.

In certain other tests which have been made in another place, there were some tendencies to hunting, and a dash-pot was applied, and the oil in this dash-pot, in several tests, was made successively thicker and thicker, which improved the conditions, but they were still not perfectly satisfactory. Then the arm of the dash-pot was increased, and was made three or four times as great, as I now recall, and the successive grades of oil were then put in, with improving results each time. Each one of some half-dozen or more tests showed an improvement, all in the way

of more effective damping, until the operation was fully satisfactory.

The matter of dampers has been referred to, and while the point has not been brought out specifically this evening, I have noticed at other times a very great tendency of the engine man to think that everything should be cured by something in the electrical system—usually dampers, more dampers; just as the electrical man thinks something is the matter with the engine and specifies dash-pots; more dash-pots!—which results in a kind of reciprocal action which keeps up a continual oscillation. The damper does not remove the cause of the disturbance, but does something which obscures it, something which prevents a cumulative action by giving a magnetic friction, a source of loss, and there are some cases in which conditions are very nearly balanced, where the damper may be of considerable use; and they turn the balance in favor of steady running. Dampers have been mentioned as being a source of loss in certain classes of dynamos. The damper will not be a source of loss if there is steady running, or if the magnetic circuit is pretty uniform. Primarily the

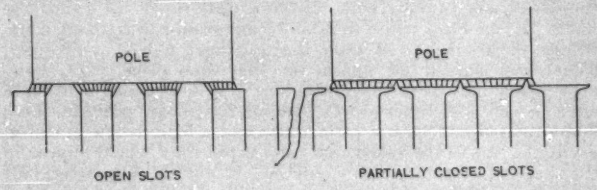


FIG. 8.

damper simply resists oscillation, and if there is uniform motion, and if the damper works effectively, it prevents the oscillation and therefore prevents the tendency to cumulative action and has no loss in it.

Returning to the mechanical analogue we may assume that the flexible shaft which is driven through gears by two engines is provided with a dash-pot. This dash-pot is so arranged as to resist angular flexure of the shaft. It will, therefore, damp the oscillations which may be set up, and thereby resist any tendency which may exist for periodic variation in the angular speed of the engines. The dash-pot acts only when there is oscillation, and does not consume energy unless there is oscillation.

The presence of dampers, however, may be a source of loss if the magnetic flux from the armature is not uniform. This may occur with armatures in which the slots between the teeth are pretty wide, so that the individual slot is wide in proportion to

the air-gap. The magnetic field near the surface of the armature is not uniform—the magnetic lines come out from the teeth in little tufts; there is a great tendency for eddy currents in the faces of the field poles or in any copper damper so placed as to have these tufts of magnetism sweeping across it. If, however, the magnetic circuit is nearly closed, or partially closed, so that at the armature surface the slots or openings are very narrow, then the magnetic flux is substantially uniform, and that loss does not occur.

Teeth of the two kinds are shown in Fig. 3. The open slots produce a magnetic field which is not uniform, and which will, therefore, tend to produce currents in the pole face after the manner of an inductor dynamo. The partly closed slots give a practically uniform field.

Rotary converters are usually made with open slots, but as there is a large number of teeth in the armature of the rotary converter, and the slots are small, the width of the slot is not large in comparison with the air-gap, so that the magnetic field reaching the pole faces, or the shields, is substantially uniform.

THE PRESIDENT:—Gentlemen, we desire now to hear something more on another type of prime mover which has not been mentioned at all here; that is, the gas engine. Perhaps Mr. Nash can give us a few words on the gas engine and its regulation. As you know, a gas engine usually has a very bad reputation, of having a greater variation of speed than even the steam engine, and it would be interesting to hear what has been done in this respect.

MR. NASH:—Mr. President and Gentlemen: I replied to the invitation to address you to-night by saying that I was not sufficiently familiar with the subject to throw any light upon it. I can only say just a few words in relation to one method of governing which it seems to me may be applied to cure the conditions which we have here. I think all the trouble that is introduced by the vibration of the piston rod and by the angular action of the connecting rod can be cured by a device which we have used successfully in the direct-connected system; that is, by putting a massive governing device between the engine and the generator. That system is, in my opinion, capable of being carried out to considerable refinement, and I have only been thinking to-night a little along the lines which have been presented by the papers and by the speakers here. I can see that there is something more required in the refinement of regulation in the alternating current system than we have attempted, and I was in hopes that perhaps some of the makers of the Westinghouse engines could have given us some light upon how they have operated the Westinghouse engine, as they probably have had more experience. I am sorry to say that I have not to-night anything to add to the subject.

THE PRESIDENT:—I voice the sentiment of the last speaker, that we would like to hear, if possible, something from the Westinghouse gas engine manufacturers who, as I know, have built very large gas engines, and most of us have seen one of them in successful operation at the Pan-American Exposition. We have a number of engineers present who have undoubtedly made the topic of to-night's papers the subject of consideration, and I would like to call on Mr. Mershon.

MR. RALPH D. MERSHON:—Mr. President and gentlemen of the INSTITUTE. The thing which has most interested me in the remarks this evening has been the question of possible electro-mechanical resonance, and it seems to me that it is a subject which, to some extent at any rate, has been avoided. The opinion has been expressed by some of the gentlemen presenting papers, that such resonance seldom if ever occurs in practice. Some others seem to think that it does occur at times. But in neither case, has any very good reason been given. If it is possible that such resonance should occur, then it seems to me that in such case the remedy which Mr. Emmet suggests for the prevention of hunting would not be effective, because, as I understand it, the remedy which he proposes is a remedy for the hunting difficulties resulting from the governors themselves, and could not affect troubles due to electro-mechanical resonance between the generators and the fly-wheels. Personally, I have never come across a case where trouble from hunting could be laid to electro-mechanical resonance, and for that reason, perhaps more than anything else, I am rather skeptical as to its ever existing in practice.

There is one point mentioned by Mr. Steinmetz, and incidentally by one or two of the gentlemen speaking after him, in regard to the stability of division of load obtained in the case of alternators in multiple by allowing a certain decrease in speed from no load to full load. This is a point of some importance which I do not think is understood by engineers generally, and is very easily and simply explained by a very simple diagram familiar to a good many of us, but not to some, perhaps. The instability of regulation due to equality of speed is well exemplified in the case of alternating generators driven by synchronous motors, and Mr. Stott, I believe, in a paper read some time ago before this institution, mentioned a case of that sort in which rather drastic measures had been resorted to in order to get the machines to properly divide their loads. As I said before, a simple diagram will show why a fall from speed will result in a stability in the division of load.

Suppose the regulation of the prime mover to be such that for equal increments of load the speed falls off equal amounts. If we plot loads on a horizontal axis and speeds on a vertical axis, the speed-load characteristic of the prime mover will be a straight line as for instance the line Y B in Fig. 1. Any point on this line corresponds to a certain definite speed, and a certain defin-

ite load; that is, when running at any given speed the prime mover must be carrying a definite corresponding load, and vice versa. Suppose there are two prime movers whose speed-load characteristics are Y B and Y C respectively, and that each drives an alternator, the alternators being in parallel. Y B and Y C are shown as not identical, because it is not possible to adjust the governors of two prime movers so that there will be the same fall speed from no load to full load. Suppose the possible accuracy of identical adjustment to be equal to the vertical distance between the points B and C. Now the prime movers must run at the same speed since their alternators are in parallel. That is, the operating points on their respective characteristics must lie on a line o b

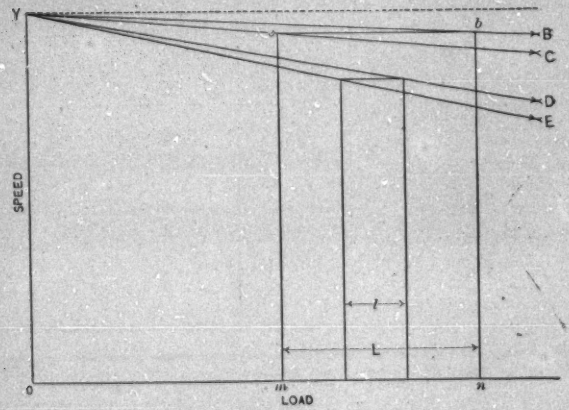


FIG. 1.

parallel to o x and the loads corresponding to the two operating points are o *m* and o *n* respectively. The difference of load carried by the machines is therefore L. Now let both governors be adjusted for a greater fall of speed, so that Y B becomes Y D and Y C becomes Y E, the vertical distance between D and E being the same as between B and C since the accuracy of identical adjustment remains about the same no matter what the fall of speed. In this latter case it will be noted that the difference in the loads carried will be only L. In general, the greater the fall in speed from no load to full load, the more nearly equal will be the loads carried by the prime movers.

Another point mentioned by Mr. Steinmetz was the fact that with an engine which governs very closely at light loads and not

so closely at heavy loads, the division of load would be more stable at heavy than at light loads. I have known of one or two cases where, without making any close determination, apparently the speed characteristics of the engines, instead of being anywhere near straight lines, were something like those of Fig. 2. You can see that in a case of this sort a given accuracy of identical adjustment of the governors will result in a less difference in load on the two engines at heavy loads than at light loads. In the engines mentioned the division was not so good at light loads as at heavy. I might mention also that in some cases where a remedy had to be applied immediately, without any chance for experimentation or

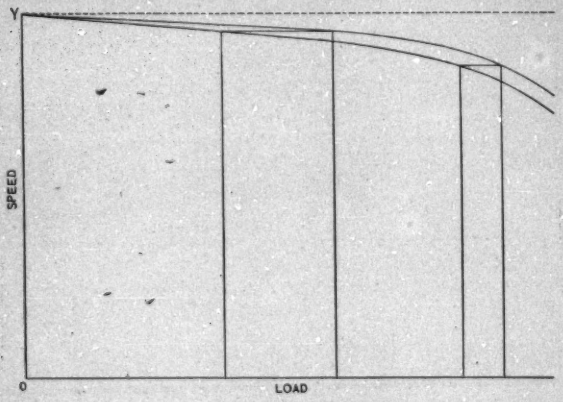


FIG. 2.

anything else, in the way of running in multiple generators which hunted with the governors adjusted as sent out by the engine builders, the trouble has been remedied by making the engines intentionally fall off a great deal more from no load to full load than they were originally intended to do.

Mr. Emmet mentioned as a probable influence in the prevention of electro-mechanical resonance that it was not likely, in most cases, that the natural period of the masses concerned would be anywhere near that of the pulsation of the engine; another reason it seems to me why there is not much chance for such vibration is the friction in the mechanisms itself which tends to deaden those vibrations, and which would correspond to the electrical friction, if we may so term it, that you would get by the use of shields or deadeners, or electrical devices of that sort for holding the machines in step.

There was one point mentioned by Mr. Keilholtz in regard to the measurement of angular velocities and variations in angular velocities. He stated that it had been suggested that a direct current machine might be driven from the prime mover, the electromotive force of the machine opposed by a storage battery, and a voltmeter used to read the difference. It seems to me a much more accurate result might be gotten by using the direct-current machine and the storage battery, but using instead of the voltmeter, an instantaneous contact device in connection with a potentiometer.

As I stated before, the point which has interested me the most, and which I am sorry has not been more gone into, is the question of electro-mechanical resonance. I wish that the gentlemen had not limited themselves to a mere expression of opinion in regard to it. I should like to have more thoroughly their reasons for thinking that such will not exist, or such will exist, in practice. Some seem to think that it is more likely to exist if a heavy fly-wheel be used While I have not gone into the matter very deeply, my opinion is rather in the opposite direction and such experience as I have had has been in the opposite direction. I have had the trouble that Mr. Emmet has spoken of in regard to the surging of two machines which was traced to the governors. In a number of cases, in one case especially, this trouble was remedied by a change in the action of the governor, not by damping it at all, but by changing its period, and that change was effected by decreasing the value of the adjusting weight on the governor and at the same time moving it farther out on the lever, so that while the moment of the adjusting weight was the same its moment of inertia was changed, and with it the natural period of the governor.

MR. EMMET:—There are just a few words I would like an opportunity to say before I go. Mr. Scott has brought up a point that Mr. Mershon has also mentioned, about the possibility of getting over this trouble by adjusting the weight to the natural period of swing of the governor. I should think that would possibly result in benefit in many cases. Adjustments could be made which would tend to prevent this trouble, which is a somewhat exceptional trouble that does not occur in all cases, and which is liable to be broken up by almost any change in condition which would affect the period. But, generally speaking, it is not advantageous to have to use any special adjustments to accomplish this result. What we want is machines that will work in parallel every time. My experience has indicated that this method, which I have advocated, accomplishes the desired result every time. I have yet to find a case where it fails, although, as I have said, there may be such cases. I should like very much to have it thoroughly tried. I have heard of many cases of parallel operating alternators that had given trouble, and are giving trouble now, more or less, at certain times. It is very desirable for the whole

electrical profession that, if a method of this sort has merits, it should be used and its merits or difficulties developed and acknowledged. We meet engine builders and people who are not in the electrical profession who do not know about these things. If electrical men do not agree, we cannot convince engine builders, and we will have very great difficulties in getting the practical results we desire. I am very anxious to hear of cases where this particular method, when properly applied, has failed.

There is one other point that Mr. Mershon brought up, that is the question of the possibility of electro-mechanical oscillation without actuation by the governor. We know, of course, that this trouble with the governor is simply an accompaniment of the tendency to oscillate electro-mechanically in a certain period by the generator. That natural tendency to oscillation is simply kept alive by the unequal delivery of the steam. I think, however, that there is a reason, which I stated briefly, and which possibly Mr. Mershon did not catch, for supposing that engines as a rule will not oscillate with their governors fixed; namely, that the engines are tied up to the reciprocating system, which has irregular friction. It is not a progressive friction, it is not a steady friction, but it is an irregular friction. Also irregular momentum which comes in jerks, and those jerks being unevenly distributed will not conform to any tendency to oscillation. We know that friction alone does not stop hunting in electrical operations. Simply applying a brake to a hunting synchronous motor will not stop its hunting at all. It apparently has no effect so far as I have seen. However, an interrupted friction, or any periodic disturbance that is not in conformity with the natural period of oscillation, will interfere with this hunting and may instantaneously stop it. Therefore, we would expect this irregularity of motion and load, imposed by the moving parts of the engine without steam, to cause a deadening effect and to discourage the electrical swing, which is a delicate, sensitive effect, with a fixed period, which, if disturbed, will disappear, as all waves will disappear, when the condition for their existence is not maintained.

THE PRESIDENT:—I think I can answer some questions raised by Mr. Mershon. I do not believe the heavy fly-wheel has any direct effect on the surging due to electro-mechanical resonance. But the vicious effect of a heavy fly-wheel is rather due to the increased tendency of the governor to overrun, because if the governor is caused to act by a temporary change of speed, due to a sudden change of load, while the temporary change of speed is reduced by the momentum of the fly wheel, the corrective effort of the governor in restoring the speed is retarded also; and therefore the governor continues to act after it ought to have stopped acting. Hence the fly-wheel has a similar effect to the momentum of the water in a hydraulic plant.

In regard to the question of electro-mechanical resonance, I have never observed it between alternators, but I have observed

very pronounced cases of electro-mechanical resonance under conditions where any other source of surging was excluded; that is, one alternator driven by a water-wheel, and one synchronous motor or converter, in a case where the frequency was absolutely uniform, where there was no hunting between governors possible. This led me to an investigation which I made about two years ago but which I never quite completed. You will find in textbooks that if you have one alternator and one synchronous motor connected by a line of certain resistance and inductance that the synchronous motor, in its electromotive force, adjusts itself in a permanent and fixed relation to the electromotive force of the generator, which relation depends on the load and the circuit and relative e. m. f's., etc. But theoretical investigation showed me that this generally accepted diagram does not represent the actual condition, but that the phase relation of a synchronous motor, operated from a single alternator at absolutely steady alternator frequency, is not definite, but may vary periodically synchronously with a variation of speed and the momentum of the synchronous apparatus, and that the period of this periodic variation of phase relation, which means periodic variation of current and voltage, or surging, depends on the constants of the electric circuit and the momentum of the moving masses; that is, this periodic variation is due to electro-mechanical resonance; and furthermore, I found that if no damping devices exist, consuming energy proportional to the variation of the speed from synchronism, then the amplitude of this periodic variation is either infinity or zero, depending on the conditions of the system. In the first case, surging occurs of cumulative character; in the last case the system is stable. Now, I have not gone any further in the investigation but shall carry it out when I have the time, since it gives the theory of electro-mechanical resonance and gives the conditions producing or eliminating it.

MR. MERSHON:—Any interaction which may take place between prime movers and their generators due to the governors is not what I understand as electro-mechanical resonance. Electro-mechanical resonance, as I understand it, involves only the moments of inertia of the prime movers and generators, the electrical properties of the generators and the speed as affecting the period of the pulsating forces on the piston. That is, true electro-mechanical resonance would exist independent of the governing mechanisms and would have place if the governors were put entirely out of action. Mr. Emmet considers the remedy he mentions to be a cure for all surging troubles, yet, as I understand it, he believes electro-mechanical resonance, as I have defined it, can exist. I do not understand how his remedy, affecting only the governor, can be a cure for that for which the governor is not responsible, and how, therefore, Mr. Emmet's remedy can be effective in case of true electro-mechanical resonance. The fact that this remedy has been effective in all cases where it was applied

seems to me evidence that in none of them was the trouble due to electro-mechanical resonance, and strengthens me in my opinion, previously expressed, that it does not have place in practice, or if so, very rarely.

THE PRESIDENT:—It is customary at the end of a discussion to ask the readers of the papers to close the discussion by answering questions. The time, however, is so far advanced to-night that I believe we may dispense with that, and ask the authors of the papers to close the discussion in writing by communicating to the Secretary the answers to the questions which have been asked of them, and give us any further remarks they desire to make. I believe a motion to adjourn would be in order.

On motion of Mr. Mailloux, the meeting adjourned at 11.15 P. M.

[COMMUNICATED AFTER ADJOURNMENT BY MR. B. A. BEHREND.]

Permit me to add the following remarks in discussing the question of operating alternators in parallel:

The question of operating alternators in parallel requires the consideration of several factors, which are, to a certain extent, contradictory of one another. Let us first consider two or more alternators driven by water-wheels, the angular velocity of which may be assumed as uniform. In order to obtain as perfect parallel operation as possible, it would seem advantageous, at first sight, to use alternators so designed that the momentum of the revolving parts is as small as possible. In this case, if the prime mover showed a tendency to run faster or slower, the second generator could, on account of its small momentum, almost instantaneously respond to these changes in the speed of the first prime mover. But, at the same time, it becomes evident that the small momentum of the alternator permits of a larger variation in angular velocity of the prime mover, and so we see that diminishing the momentum of the rotating parts of the alternator would be advantageous in one respect and disadvantageous in another, and it becomes a question of compromise to strike a balance between the advantages and disadvantages. From the above reasoning it is also plain that a very large momentum would prevent the alternator revolving at a uniform angular velocity, from responding to the changes in angular velocity in the other alternator, the prime mover of which is, for the time being, lagging or leading, and therefore the exchange of synchronizing current might be greater with a heavy fly-wheel than with a light one, although the heavy fly-wheel would diminish the lack of uniformity in angular variation of the prime mover.

It is, of course, imperative to obtain an angular velocity as nearly uniform as possible. We may consider three different kinds of prime movers:

(1) Water-wheels and steam turbines.
(2) Steam engines.
(3) Gas and oil engines.

The prime mover under (1) has practically a uniform angular velocity. The prime mover under (2) receives a number of impulses during one revolution which is dependent upon the number of cranks of the engine. The prime movers under (3) are the most unfavorable in regard to uniform angular velocity, owing to the explosions of the gas and the irregular crank-effort consequent upon it. In steam and gas engines driving alternators, it is a question of great importance to carefully consider the curve of

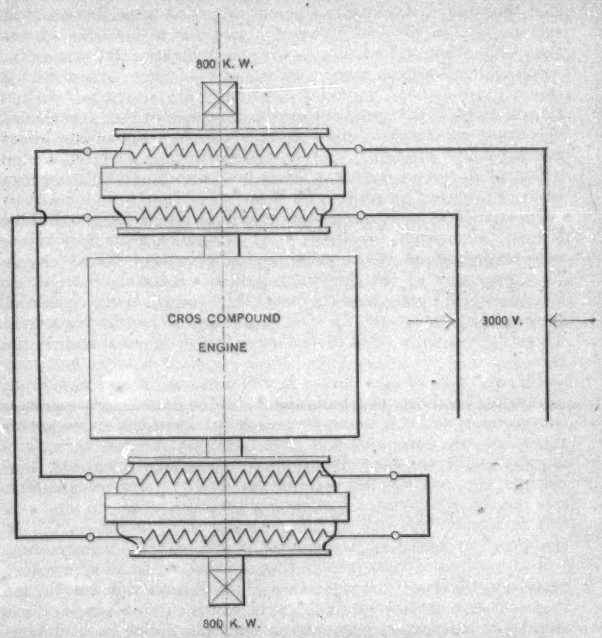


Diagram showing series connection of two 800 k. w. inductor generators on the same shaft. (Two phases only are shown.)

crank-effort and to make the crank-effort as constant as possible. This can be done by adopting two or more cranks properly displaced towards each other, and by carefully taking into consideration the momentum of the reciprocating parts and the influence on the diagram of crank-effort. If all these points are duly observed, no difficulty in operating alternators in parallel will be

found, if driven by any of the prime movers mentioned under heads (1), (2) and (3). It may be well, however, to mention one phenomenon which is of paramount importance in operating alternators in parallel driven by steam or gas engines. The combination "alternator and fly-wheel" forms a dynamical system which is, in its behavior, analagous to the dynamical system formed by a spring and weight. Such a system has a natural period of vibration if once thrown into oscillation. Now let us imagine that the impulses from the prime mover will be so timed that their period is equal to the natural period of oscillation of the system "alternator and fly-wheel," and you will readily see that the oscillations, at first small, will be tremendously exaggerated by the impulses from the prime mover. In order to prevent this case of "resonance," to which Prof. A. Blondel first called attention in 1892, it is incumbent on the designer of the generator to determine the natural period of oscillation of his generator and to compare this with the "forced oscillation" caused by the impulses of the prime mover. These two periods of oscillation must be as different as possible. To illustrate:

In some 600 k. w. generators that I designed for the Bullock Electric Company, for the city of Nashville, the prime mover runs at a speed of 120 r. p. m. and is a cross-compound engine. The frequency of the engine impulses is therefore 8 cycles per second, while I computed the natural frequency of the dynamical system "generator and fly-wheel" to be 1.57 cycles per second. These figures show that there is no danger of resonance in this case.

The question of momentum in synchronous motors and rotary converters has been much discussed. It is entirely a question of compromise, and it is necessary, so to speak, to strike the golden mean between two opposing principles. A certain amount of momentum is necessary to take care of sudden overloads, short circuits, etc., but the less momentum we have, the more responsive are the synchronous motors or rotary converters to the variation in angular velocity of the generator and, therefore, the exchange of synchronizing currents is diminished in proportion. "Hunting" is a phenomenon that is inherent in all alternating current apparatus. Its causes have been discussed at length, and its prevention or diminution must be found in obtaining uniform angular velocity in the prime mover, and in avoiding rapid changes of load on the synchronous motors or rotary converters.

Steam engineers have worked over the problem of obtaining uniform angular velocity in the engines, and electrical engineers have used storage batteries in connection with rotary converters in order to avoid sudden changes of the load. But there are limits in both cases which can not be over-stepped, and therefore the *causes* of hunting can never be entirely avoided. The *symptoms* of hunting, however, can be greatly diminished by using damping coils which, though they involve a certain loss of energy,

damp out of existence the oscillation that is set up by changes in angular velocity of the prime mover, or by changes of the load.

"Hunting" of alternators driven by steam engines is often caused by the "fluttering" of the governors. In this case the remedy is simple and consists in putting an effective damping device, as a dash-pot, on the governor.

I observed a very curious case in the early days of large engine-driven alternators. The Oerlikon Co. had built four 800 k. w. inductor generators running at 107 r. p. m. for one of the large Berlin central stations. Two of these generators, which were of the double inductor type, wound for 3,000 volts and 50 cycles, were driven by one vertical steam engine. The generators had a very small synchronous inductance; their design was a reaction against the generators of the Lauffen type, of high synchronous inductance, and therefore the cross currents between two armatures, very slightly offset, were enormous. The two generators, mounted on the ends of the same shaft, *could not be operated in parallel.* Without any load, the apparent energy of the cross-currents amounted to about 1,000 k. v. a. at normal excitation. The shaft not being absolutely rigid, and the engines being cross-compound engines, the impulses from the high pressure cylinder, and those from the low pressure cylinder, were not in synchronism, leading to tremendous surging and oscillation, so that at times a breaking of the engine shaft seemed imminent. Reactance coils were tried, but without any success. It struck me that it must be possible to connect the generators in series instead of in parallel, and in order to obtain again 3,000 volts, I had only one armature of each generator connected in series with the corresponding armature of the other generator, so that we obtained the arrangement as shown by the sketch.

This remedy was perfect. The two independently driven sets of 1,600 k. w. each could be operated in parallel without any difficulty.

[Communicated after Adjournment by Mr. August H. Kruesi.]

We are indebted to Mr. Keilholtz for the results of an actual measurement of velocity variation, but it is regrettable that he does not feel sure of the value he gives. Such measurements are inherently difficult, and very few figures have so far been published. I know of some recent experiments in which the varying potential of a small direct current dynamo driven by the engine was measured by an oscillograph. The oscillograph seemed well suited to this purpose, but the fluctuation in potential, due to the finite number of commutator segments, even in the case of a smooth core machine, was found to be of even greater magnitude than the engine variations, so that the latter were completely obliterated.

The value of actual measurements of velocity variation of engines which have proved bad or just on the border line is

great, first as a check on our methods of calculation, which can hardly be considered certain at present, and second, to ascertain whether or not the degree of uniformity in speed now demanded is not excessively rigorous and our fly-wheels unnecessarily heavy. There is probably no other accurate way to compare engines of various designs or engines having different fly-wheel capacities, and until such comparisons have been made between engines which give poor parallel operation and those which operate well, the determination of fly-wheel capacity will remain largely a matter of guess work.

It seems to be pretty generally agreed that the governor should be made responsible for parallel operation, and that "the tendency to hunting seems to be aggravated by the use of heavy fly-wheels." Mr. Keilholtz's figure of 11 mm. displacement on the periphery of the wheel is equivalent to 3.9° of phase for a 25-cycle, 30-pole alternator. Since the engine drives a direct current railway generator this amount of fly-wheel is presumably necessary for railway service, and it is desirable that no more should be necessary for an alternator, but we may be quite certain that the engine builder would have used much more. European engineers have practised parallel operation from low speed engines very extensively and successfully, and with relatively light fly-wheels.

We may arrive roughly at the sufficiency of fly-wheel in any case by finding the energy in foot-pounds stored in the wheel per k. w. and pole of the alternator, and multiplying by the number of cranks, making 90° or 120° angle, since for a given wheel the larger the number of cranks the less the speed variation will be.

That is, the figure of sufficiency $= J = \dfrac{w\, v^2 \times \text{cranks}}{2\, g \times \text{k. w.} \times \text{poles}}$

Analyzing nine recent European engines in this way, I find the average figures for nine low speed engines to be 200 ft.-lbs. per k. w. and pole, the smallest being 100 and the largest 250. Four large low-speed engines of one make, recently built in America, give an average of 500, the smallest being 485 and the largest 505. Are not our wheels too heavy?

[Communicated after Adjournment by E. W. Mix, of Paris.]

Considerable attention has of late been called to the problem of measuring conveniently the variation of speed during one revolution of a fly-wheel or revolving field of an alternating-current dynamo. The apparatus described herewith is very convenient for this work. The principle is the following:

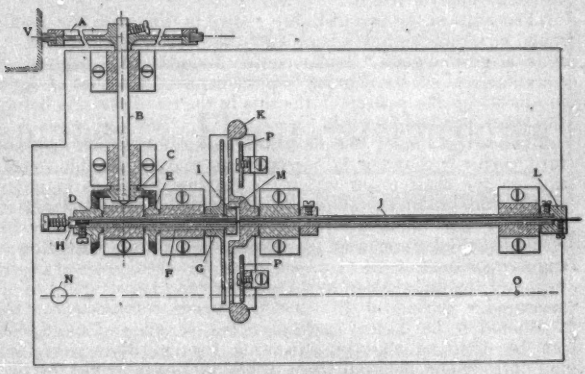
A small disk having considerable inertia is driven by means of a flexible coupling at a practically constant speed by means of the fly-wheel under observation, while a light aluminium disk is driven in the opposite direction at a speed proportional to the

speed of the fly-wheel and obliged to follow all the variation of speed of the latter. These two disks are mounted side by side, with their axes in the same line, and a radial slit is cut in each of them at the same distance from the center. When the two disks turn at exactly the same speed these two slits pass one another at the same point in space, but should one revolve faster than the other the point of passing of these slits will be displaced in one sense or the other, depending on which disk is revolving the faster.

By placing a lamp on one side of the disks and an opaque screen on the other a spot of light will be projected on this screen and its displacement can be easily observed.

If now the aluminium disk be driven with an angular velocity, say 40 times as great as that of the fly-wheel, whose variations in speed are to be observed, and the disk with high inertia be driven by means of a flexible coupling from the same fly-wheel, the relation between the inertia of the disk and the flexibility of its coupling being such that the former will revolve at practically constant speed and proportion to the mean speed of the fly-wheel, then the spot of light on the screen will move back and forth through an angle 40 times as great as the angular variation of speed of the fly-wheel. By thus multiplying the angular variations the measurements can be made with great accuracy.

The figure shown herewith is made from a scale drawing of the apparatus as actually constructed.



Apparatus for Measuring Speed Variations.

In this figure, v represents a section of the rim of the fly-wheel whose variations of speed are to be measured. The small disk, or wheel A, made with as little inertia as possible, is

provided with a rubber tire so as to be driven by friction against the rim of the fly-wheel. This wheel is mounted on one end of a shaft B, carrying at the other end a conical pinion C, which drives two other conical pinions D and E, having an equal number of teeth.

The pinion E is mounted on one end of a hollow shaft F, which carries at the other end the light aluminium disk G.

The pinion D is mounted on one end of the hollow shaft H, passing inside the shaft F and carrying at the other end a collar provided with a lug I.

The disk K, provided with a heavy rim, so as to give it considerable inertia, is mounted on one end of the hollow shaft J, the latter being mounted in line with the shaft F. The other end of J is provided with a collar L, which can be fixed on the shaft, by means of a screw, in any angular position.

A small steel wire passes through the two hollow shafts H and J, one end being fastened by a screw in H, the other being held in the collar L. By twisting more or less this steel wire by means of the collar L, the necessary turning moment for driving the disk K can be obtained.

Inside the cavity in the hub of the disk K is a little lug M which can come in contact with the lug I fixed to the end of the shaft H. By means of these two lugs the disk K can be brought up to speed without undue strain on the steel wire which normally drives it.

The slits in the two disks are shown in line with the points N and O, N representing a lamp and O the eye of the observer.

P is a ground-glass circular scale divided into degrees of a circle on which the angular displacements of the spot of light produced by the passage of the slits in the two disks, one before the other, can be observed.

In case the rim of the fly-wheel under observation does not turn perfectly round it is necessary to make a flexible extension to the shaft B, on which the driving disk A is mounted so that this disk can follow the rim without being obliged to compress too much the rubber tire, as in the latter case the resultant circumference of the disk A would vary at different angular positions of the fly-wheel, and so produce errors of observation of the same order as the angular variation of speed to be measured.

Should it be desired to register the variation of speed this can be done by slightly separating the two disks G and K, and introducing between them a satellite pinion driven by two conical pinions, one mounted on each of the disks. An index carried by the axis of this satellite pinion can be made to describe a curve on a piece of paper driven before it at a uniform speed, as is done in a recording ampere-meter.

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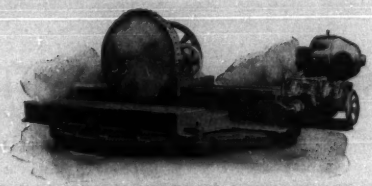

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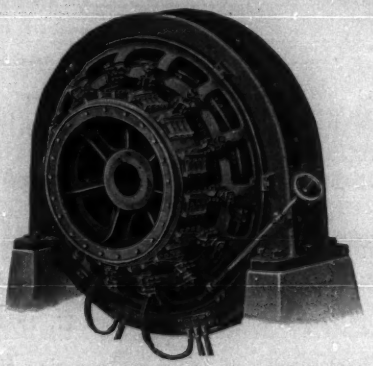

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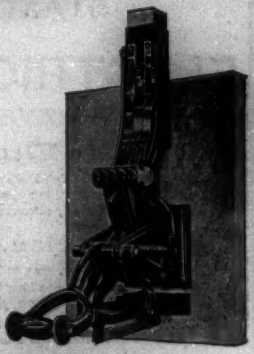

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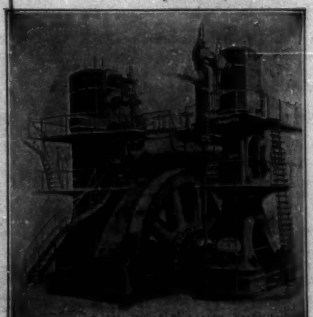

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